PROCEEDINGS

OF THE

Cambridge Philosophical Society.

VOLUME IV.

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CAMBRIDGE PHILOSOPHICAL SOCIETY.

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OCTOBER 25, 1880—MAY 28, 1883.

Cambridge:

PRINTED AT THE UNIVERSITY PRESS.

AND SOLD BY

DEIGHTON, BELL AND CO. AND MACMILLAN AND CO. CAMBRIDGE;
BELL AND SONS, LONDON.

1883

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PROCEEDINGS

OF THE

Cambridge Philosophical Society.

ANNUAL GENERAL MEETING.

October 25, 1880.

PROFESSOR NEWTON, PRESIDENT, IN THE CHAIR.

The following were elected Officers and new members of Council for the ensuing year :—

President.

Professor Newton.

Vice-Presidents.

Professor Liveing.
Mr Michael Foster.
Dr W. M. Campion.

Treasurer.

Dr J. B. Pearson.

Secretaries.

Mr J. W. Clark.
Mr Coutts Trotter.
Mr J. W. L. Glaisher.

New Members of Council.

Professor Stokes.
Lord Rayleigh.
Mr S. H. Vines.

Captain PHILIP GOING, R.N., was balloted for, and duly elected an associate of the Society.

The PRESIDENT opened the proceedings with the following remarks: Having now completed my year of office you will perhaps think it becoming of me to say a few words on the present occasion of our annual general meeting. I trust that during the twelve months that I have been your president the Society has had no reason to regret its choice. Beyond, however, returning my sincere thanks to the Council and all the members of the Society—but especially to the Treasurer and Secretaries—for the kindness which I have invariably received at their hands, in the discharge of my duties, I will not occupy your time by dwelling at any length on the events of the past year. What I have to say refers more to the future.

First I would remark that the attendance at our meetings, though I have no reason to believe it has generally fallen below the average of the last few years, is certainly not so full as is to be desired, and is, if I mistake not, considerably smaller than it used to be in the days when I first knew the Society. It seems to me that there is a very easy explanation of this fact. In former days the customary hour of dinner in the University was much earlier than it now is; and, more than that, all or nearly all of the Colleges dined at almost the same hour. Now, as you are aware, there is a very great diversity in this respect, and this change of habits appears to me to be obviously the prime cause of the small attendance. I believe I am right in stating that those Colleges which maintain the ancient practice of dining at a comparatively early hour are now in a minority—certainly they are in a minority as regards the number of members belonging to them. I speak in the presence of those who will correct me if I am in error, but I think that the hour of the Society's meetings has more than once been changed in past time, and that we meet now at a later hour than formerly. I would therefore venture to suggest that the Society might find it advantageous to consider this subject once more; but it will be apparent, I think, to all that we cannot fix a later hour than we have at present; and that, if any change be made, it must be to one that is earlier. It will be within the knowledge of several here present that some months ago the Royal Society determined to try the experiment of holding its meetings at an earlier hour—before instead of after dinner. The experiment has, I believe, succeeded, I will not say to the full extent that some expected, but at all events partially. I understand that the attendance has improved, and that the new arrangement has been found convenient by the most constant frequenters of the Society's meetings, whether they be officials or not. I therefore cannot help throwing out the hint that we might find it expedient to try the same experiment in Cambridge, and to hold our meetings in the course of the afternoon.

The next point upon which I will touch is one that is likely to e of great importance to the Society. Fellows are aware that uring the past Vacation, some long-projected alterations of the uilding in which we are assembled have been completed, with he result of throwing into one two smaller rooms. The large hamber thus formed is at present unoccupied, but it is no secret for the assertion was made openly in the Schools) that the intenion of some of those who brought about this change was to proide a library for the accommodation of scientific books for the se of all those who use this building. The fact that such a ibrary would sooner or later become necessary has been long oreseen, and, for myself, the time of need seems now to have rrived. This proposal has many times been discussed in private, nd I believe it has been openly urged that the library of the Society should form the nucleus of the new collection. A week go the subject was brought formally before the Council of the Society, and a Committee appointed to report thereon. It will be lain to all that the Society would be a great gainer if its books ould be accommodated in a more accessible room than that vhich they now occupy, and members would without doubt find he large chamber on the ground floor very commodious for their urposes. But, on the other hand, there are necessarily some isadvantages, supposing that this plan was carried out. In the rst place, the books thus being so much more accessible would ave to be put under a much stricter supervision than at present, nd this supervision could not be attained without some expense. Then, too, it has been announced that this large room will require o be used for examinations, and it is of course obvious that in hat case unrestricted admission could not be at all times enjoyed ven by Fellows of the Society; while we all know, from the xperience of past years, the tendency of examinations to increase oth in number and in duration. Fellows of the Society might herefore come to find that the projected change would involve a erious deprivation of their rights. Yet, with all this, I fully elieve that the Society would on the whole benefit by the alteraion, and I trust that means may be found whereby the inconenience I have mentioned may be reduced to a minimum. More han this it is impossible for me to say at present, and of course t will be understood that the Society will have due notice given o it of any action taken by the Council, and an opportunity of xpressing its opinion thereon, but I have thought it only right to ake the present opportunity of announcing to the Society at this neeting what is in contemplation, and the possibility of some rrangement being entered into with the University by the Council ubject to the Society's approval.

There is one other matter I should like to mention to the Society. By our Bye-Laws (chap. xii.) it is laid down that "the common seal, charter and deeds of the Society shall be kept in an iron chest with two locks and two different keys, the one to be kept by the President, and the other by the Treasurer." We have the chest: the Treasurer and I hold the keys prescribed by the law, and the Society's seal and certain documents are safely kept therein: but our charter is not forthcoming, and no one of the present or former officers with whom I have been able to confer can throw any light upon what is become of it. This announcement I make to the Society, not merely to exonerate myself and your present officers from responsibility, but rather in the hope that one result thereof may be the recovery of this instrument, which is interesting and valuable to us, and of no use to any one else.

The following communications were made to the Society:

(1) *On the general motion of a liquid ellipsoid under the gravitation of its own parts; continuation of a paper on the rotation of a liquid ellipsoid* (Vol. III. pp. 289—293). By A. G. Greenhill, M.A., Fellow of Emmanuel College.

The following paper contains a new method of solving the problem, by means of moving axes, which has already been attacked by Lejeune-Dirichlet and Riemann, in the *Abhandlungen der Königl. Gesellschaft der Wissenschaften zu Göttingen*, in the 8th and 9th volumes respectively.

Suppose the ellipsoid $\frac{x^2}{a^2}+\frac{y^2}{b^2}+\frac{z^2}{c^2}=1$ to be filled with liquid and suppose the liquid frozen, and the ellipsoid to have component angular velocities ξ, η, ζ.

Then if u, v, w denote the component velocities at xyz, parallel to the axes,

$$u=-y\zeta+z\eta,\quad v=-z\xi+x\zeta,\quad w=-x\eta+y\xi.$$

If the liquid now be melted, and additional angular velocities Ω_1, Ω_2, Ω_3 communicated to the ellipsoid about its axes, then

$$\left.\begin{aligned} u&=-y\zeta+z\eta+\Omega_2\frac{c^2-a^2}{c^2+a^2}z+\Omega_3\frac{a^2-b^2}{a^2+b^2}y,\\ v&=-z\xi+x\zeta+\Omega_3\frac{a^2-b^2}{a^2+b^2}x+\Omega_1\frac{b^2-c^2}{b^2+c^2}z,\\ w&=-x\eta+y\xi+\Omega_1\frac{b^2-c^2}{b^2+c^2}y+\Omega_2\frac{c^2-a^2}{c^2+a^2}x;\end{aligned}\right\}\quad\ldots\ldots\ldots\ldots(A)$$

ıd if U, V, W denote the component velocities of the liquid *rela-ve to the ellipsoid,* then

$$\left.\begin{aligned} U &= u + y\omega_3 - z\omega_2 = \frac{2a^2}{a^2+b^2}\Omega_3 y - \frac{2a^2}{c^2+a^2}\Omega_2 z, \\ V &= v + z\omega_1 - x\omega_3 = \frac{2b^2}{b^2+c^2}\Omega_1 z - \frac{2b^2}{a^2+b^2}\Omega_3 x, \\ W &= w + x\omega_2 - y\omega_1 = \frac{2c^2}{c^2+a^2}\Omega_2 x - \frac{2c^2}{b^2+c^2}\Omega_1 y; \end{aligned}\right\}\quad\text{.........}(B)$$

here ω_1, ω_2, ω_3 are the component angular velocities of the ellip-id about its axes, and therefore

$$\omega_1 = \Omega_1 + \xi, \quad \omega_2 = \Omega_2 + \eta, \quad \omega_3 = \Omega_3 + \zeta.$$

We see that $U\dfrac{x}{a^2} + V\dfrac{y}{b^2} + W\dfrac{z}{c^2} = 0$, and therefore a liquid par-cle always remains on a similar ellipsoid.

If h_1, h_2, h_3 denote the components of angular momentum about ıe axes,

$$\left.\begin{aligned} h_1 &= \Sigma m\,(wy - vz) \\ &= \Omega_1 \frac{b^2-c^2}{b^2+c^2}\Sigma m\,(y^2 - z^2) + \xi\Sigma m\,(y^2+z^2) \\ &= \tfrac{1}{5}M\left\{\frac{(b^2-c^2)^2}{b^2+c^2}\Omega_1 + (b^2+c^2)\,\xi\right\} \\ h_2 &= \tfrac{1}{5}M\left\{\frac{(c^2-a^2)^2}{c^2+a^2}\Omega_2 + (c^2+a^2)\,\eta\right\} \\ h_3 &= \tfrac{1}{5}M\left\{\frac{(a^2-b^2)^2}{a^2+b^2}\Omega_3 + (a^2+b^2)\,\zeta\right\} \end{aligned}\right\}\quad\text{............}(C),$$

I denoting the mass of the liquid.

If no external forces act, the dynamical equations are

$$\left.\begin{aligned} \frac{dh_1}{dt} - h_2\omega_3 + h_3\omega_2 &= 0, \\ \frac{dh_2}{dt} - h_3\omega_1 + h_1\omega_3 &= 0, \\ \frac{dh_3}{dt} - h_1\omega_2 + h_2\omega_1 &= 0; \end{aligned}\right\}\quad\text{........................}(D)$$

ıd therefore $h_1^2 + h_2^2 + h_3^2 = G^2$, a constant.

Also the kinetic energy

$$T = \tfrac{1}{2}\Sigma m(u^2+v^2+w^2)$$

$$= \tfrac{1}{10}M\left\{\frac{(b^2-c^2)^2}{b^2+c^2}\omega_1^2 + \frac{(c^2-a^2)^2}{c^2+a^2}\omega_2^2 + \frac{(a^2-b^2)^2}{a^2+b^2}\omega_3^2 + \frac{4b^2c^2}{b^2+c^2}\xi^2 + \frac{4c^2a^2}{c^2+a^2}\eta^2 + \frac{4a^2b^2}{a^2+b^2}\zeta^2\right\},$$

a constant.

The hydrodynamical equations with moving axes are

$$\left.\begin{aligned}
\frac{1}{\rho}\frac{dp}{dx} - X + \frac{du}{dt} - v\omega_3 + w\omega_2 + U\frac{du}{dx} + V\frac{du}{dy} + W\frac{du}{dz} &= 0,\\
\frac{1}{\rho}\frac{dp}{dy} - Y + \frac{dv}{dt} - w\omega_1 + u\omega_3 + U\frac{dv}{dx} + V\frac{dv}{dy} + W\frac{dv}{dz} &= 0,\\
\frac{1}{\rho}\frac{dp}{dz} - Z + \frac{dw}{dt} - u\omega_2 + v\omega_1 + U\frac{dw}{dx} + V\frac{dw}{dy} + W\frac{dw}{dz} &= 0;
\end{aligned}\right\} \quad \ldots\ldots(E)$$

and by the elimination of the pressure and the potential, supposing one to exist,

$$\left.\begin{aligned}
\frac{d\xi}{dt} - \eta\omega_3 + \zeta\omega_2 + U\frac{d\xi}{dx} + V\frac{d\xi}{dy} + W\frac{d\xi}{dz} &= \xi\frac{du}{dx} + \eta\frac{du}{dy} + \zeta\frac{du}{dz},\\
\frac{d\eta}{dt} - \zeta\omega_1 + \xi\omega_3 + U\frac{d\eta}{dx} + V\frac{d\eta}{dy} + W\frac{d\eta}{dz} &= \xi\frac{dv}{dx} + \eta\frac{dv}{dy} + \zeta\frac{dv}{dz},\\
\frac{d\zeta}{dx} - \xi\omega_2 + \eta\omega_1 + U\frac{d\zeta}{dx} + V\frac{d\zeta}{dy} + W\frac{d\zeta}{dz} &= \xi\frac{dw}{dx} + \eta\frac{dw}{dy} + \zeta\frac{dw}{dz};
\end{aligned}\right\} \quad ..(F)$$

where ξ, η and ζ have their usual meaning.

With our values of u, v and w, these equations (F) reduce to

$$\left.\begin{aligned}
\frac{d\xi}{dt} &= \frac{2a^2}{a^2+b^2}\Omega_3\eta - \frac{2a^2}{c^2+a^2}\Omega_2\zeta,\\
\frac{d\eta}{dt} &= \frac{2b^2}{b^2+c^2}\Omega_1\zeta - \frac{2b^2}{a^2+b^2}\Omega_3\xi,\\
\frac{d\zeta}{dt} &= \frac{2c^2}{c^2+a^2}\Omega_2\xi - \frac{2c^2}{b^2+c^2}\Omega_1\eta,
\end{aligned}\right\} \quad \ldots\ldots\ldots\ldots\ldots\ldots(G)$$

three equations, similar to equations (B); ξ, η, ζ having the same values as in equations (A).

These equations may be written

$$\frac{1}{a}\frac{d\xi}{dt} - \frac{2ab}{a^2+b^2}\Omega_3\frac{\eta}{b} + \frac{2ca}{c^2+a^2}\Omega_2\frac{\zeta}{c} = 0,$$

$$\frac{1}{b}\frac{d\eta}{dt} - \frac{2bc}{b^2+c^2}\Omega_1\frac{\zeta}{c} + \frac{2ab}{a^2+b^2}\Omega_3\frac{\xi}{a} = 0,$$

$$\frac{1}{c}\frac{d\zeta}{dt} - \frac{2ca}{c^2+a^2}\Omega_2\frac{\xi}{a} + \frac{2bc}{b^2+c^2}\Omega_1\frac{\eta}{b} = 0.$$

We see that

$$\frac{\xi}{a^2}\frac{d\xi}{dt} + \frac{\eta}{b^2}\frac{d\eta}{dt} + \frac{\zeta}{c^2}\frac{d\zeta}{dt} = 0,$$

nd therefore

$$\frac{\xi^2}{a^2} + \frac{\eta^2}{b^2} + \frac{\zeta^2}{c^2} = \text{constant}.$$

The first equation of (E) becomes now, supposing the internal orces due to the mutual gravitation of the liquid particles, and utting

$$A = \tfrac{3}{2}M\int_0^\infty \frac{d\lambda}{(a^2+\lambda)P}, \qquad B = \tfrac{3}{2}M\int_0^\infty \frac{d\lambda}{(b^2+\lambda)P},$$

$$C = \tfrac{3}{2}M\int_0^\infty \frac{d\lambda}{(c^2+\lambda)P},$$

here $$P^2 = (a^2+\lambda)(b^2+\lambda)(c^2+\lambda);$$

$$\frac{1}{\rho}\frac{dp}{dx} + Ax - y\dot{\zeta} + 2\dot{\eta} + \frac{c^2-a^2}{c^2+a^2}\dot{\Omega}_2 z + \frac{a^2-b^2}{a^2+b^2}\dot{\Omega}_3 y$$

$$-\left(-z\xi + x\zeta + \frac{a^2-b^2}{a^2+b^2}\Omega_3 x + \frac{b^2-c^2}{b^2+c^2}\Omega_1 z\right)\omega_3$$

$$+\left(-x\eta + y\xi + \frac{b^2-c^2}{b^2+c^2}\Omega_2 y + \frac{c^2-a^2}{c^2+a^2}\Omega_2 x\right)\omega_2$$

$$+\left(\frac{2b^2}{b^2+c^2}\Omega_1 z - \frac{2b^2}{a^2+b^2}\Omega_1 x\right)\left(-\zeta + \frac{a^2-b^2}{a^2+b^2}\Omega_3\right)$$

$$+\left(\frac{2c^2}{c^2+a^2}\Omega_2 x - \frac{2c^2}{b^2+c^2}\Omega_1 y\right)\left(\eta + \frac{c^2-a^2}{c^2+a^2}\Omega_2\right) = 0,$$

nd the coefficient of y in this equation

$$= -\dot{\zeta} + \frac{a^2-b^2}{a^2+b^2}\dot{\Omega}_3 + \left(\xi + \frac{b^2-c^2}{b^2+c^2}\Omega_1\right)(\Omega_2+\eta)$$

$$- \frac{2c^2}{b^2+c^2}\Omega_1\left(\eta + \frac{c^2-a^2}{c^2+a^2}\Omega_2\right)$$

$$= -\frac{2c^2}{c^2+a^2}\Omega_2\xi + \frac{2c^2}{b^2+c^2}\Omega_1\eta + \frac{a^2-b^2}{a^2+b^2}\dot{\Omega}_3$$

$$+ \left(\xi + \frac{b^2-c^2}{b^2+c^2}\Omega_1\right)(\Omega_2+\eta) - \frac{2c^2}{b^2+c^2}\Omega_1\left(\eta + \frac{c^2-a^2}{c^2+a^2}\Omega_2\right)$$

$$= \frac{a^2-b^2}{a^2+b^2}\dot{\Omega}_3 - \frac{c^2-a^2}{c^2+a^2}\Omega_2\xi + \frac{b^2-c^2}{b^2+c^2}\Omega_1\eta$$

$$- \left\{\frac{4c^4}{(b^2+c^2)(c^2+a^2)} - 1\right\}\Omega_1\Omega_2 + \xi\eta.$$

But from the third equation of the dynamical equations, (D)

$$\frac{(a^2-b^2)^2}{a^2+b^2}\dot{\Omega}_3 + (a^2+b^2)\dot{\zeta} - \left\{\frac{(b^2-c^2)^2}{b^2+c^2}\Omega_1 + (b^2+c^2)\xi\right\}\omega_2$$

$$+ \left\{\frac{(c^2-a^2)^2}{c^2+a^2}\Omega_2 + (c^2+a^2)\eta\right\}\omega_1 = 0,$$

or

$$\frac{(a^2-b^2)^2}{a^2+b^2}\dot{\Omega}_3 + 2c^2\frac{a^2+b^2}{c^2+a^2}\Omega_2\xi - 2c^2\frac{a^2+b^2}{b^2+c^2}\Omega_1\eta$$

$$- \left\{\frac{(b^2-c^2)^2}{b^2+c^2} - \frac{(c^2-a^2)^2}{c^2+a^2}\right\}\Omega_1\Omega_2 + \left\{\frac{(c^2-a^2)^2}{c^2+a^2} - (b^2+c^2)\right\}\Omega_2\xi$$

$$- \left\{\frac{(b^2-c^2)^2}{b^2+c^2} - (c^2+a^2)\right\}\Omega_1\eta + (a^2-b^2)\xi\eta = 0,$$

and reducing, and dividing by the factor a^2-b^2,

$$\frac{a^2-b^2}{a^2+b^2}\dot{\Omega}_3 - \frac{c^2-a^2}{c^2+a^2}\Omega_2\xi + \frac{b^2-c^2}{b^2+c^2}\Omega_1\eta - \left\{\frac{4c^4}{(b^2+c^2)(c^2+a^2)} - 1\right\}\Omega_1\Omega_2$$

$$+ \xi\eta = 0;$$

or the coefficient of y vanishes.

Similarly the coefficient of z vanishes, and the equation becomes

$$\frac{1}{\rho}\frac{dp}{dx}+\left\{A+\left(-\eta+\frac{c^2-a^2}{c^2+a^2}\Omega_2\right)(\Omega_2+\eta)\right.$$

$$+\left(\eta+\frac{c^2-a^2}{c^2+a^2}\Omega_2\right)\frac{2c^2}{c^2+a^2}\Omega_2-\left(\zeta+\frac{a^2-b^2}{a^2+b^2}\Omega\right)(\Omega_3+\zeta)$$

$$\left.-\left(-\zeta+\frac{a^2-b^2}{a^2+b^2}\Omega_3\right)\frac{2b^2}{a^2+b^2}\Omega_3\right\}x=0,$$

$$\frac{1}{\rho}\frac{dp}{dx}+\left\{A+\frac{(c^2-a^2)(3c^2+a^2)}{(c^2+a^2)^2}\Omega_2^{\,2}+2\frac{c^2-a^2}{c^2+a^2}\Omega_2\eta-\eta^2\right.$$

$$\left.-\frac{(a^2-b^2)(a^2+3b^2)}{(a^2+b^2)^2}\Omega_3^{\,2}-2\frac{a^2-b^2}{a^2+b^2}\Omega_3\zeta-\zeta^2\right\}x=0,$$

with two similar equations in y and z; or

$$\frac{1}{\rho}\frac{dp}{dx}+A'x=0,\qquad \frac{1}{\rho}\frac{dp}{dy}+B'y=0,\qquad \frac{1}{\rho}\frac{dp}{dz}+C'z=0;$$

where

$$A'=A+\frac{(c^2-a^2)(3c^2+a^2)}{(c^2+a^2)^2}\Omega_2^{\,2}+2\frac{c^2-a^2}{c^2+a^2}\Omega_2\eta-\eta^2$$

$$-\frac{(a^2-b^2)(a^2+3b^2)}{(a^2+b^2)^2}\Omega_3^{\,2}-2\frac{a^2-b^2}{a^2+b^2}\Omega_3\zeta-\zeta^2$$

$$=A+\frac{4c^2(c^2-a^2)}{(c^2+a^2)^2}\Omega_2^{\,2}-\left(\frac{c^2-a^2}{c^2+a^2}\Omega_2-\eta\right)^2$$

$$-\frac{4b^2(a^2-b^2)}{(a^2+b^2)^2}\Omega_3^{\,2}-\left(\frac{a^2-b^2}{a^2+b^2}\Omega_3+\zeta\right)^2$$

$$B'=B+\frac{4a^2(a^2-b^2)}{(a^2+b^2)^2}\Omega_3^{\,2}-\left(\frac{a^2-b^2}{a^2+b^2}\Omega_3-\zeta\right)^2$$

$$-\frac{4c^2(b^2-c^2)}{(b^2+c^2)^2}\Omega_1^{\,2}-\left(\frac{b^2-c^2}{b^2+c^2}\Omega_1+\xi\right)^2$$

$$C'=C+\frac{4b^2(b^2-c^2)}{(b^2+c^2)^2}\Omega_1^{\,2}-\left(\frac{b^2-c^2}{b^2+c^2}\Omega_1-\xi\right)^3$$

$$-\frac{4a^2(c^2-a^2)}{(c^2+a^2)^2}\Omega_2^{\,2}-\left(\frac{c^2-a^2}{c^2+a^2}\Omega_2+\eta\right)^2.$$

Therefore, integrating,

$$\frac{p}{\rho} + \tfrac{1}{2}(A'x^2 + B'y^2 + C'z^2) = \text{constant},$$

and therefore the surfaces of equal pressure are the similar and co-axial quadrics

$$A'x^2 + B'y^2 + C'z^2 = \text{constant}.$$

(Putting $\xi = 0$, $\eta = 0$, $\zeta = 0$, we get the particular case considered, p. 240, *Proceedings of the Cambridge Philosophical Society*, Vol. III.)

If we can make A', B', C' constant, and

$$A'a^2 = B'b^2 = C'c^2,$$

the surfaces of equal pressure are similar to the external surface.

A free surface would therefore be possible, and the external case might be removed.

We should then have a liquid ellipsoid moving under the gravitation of its parts, but not about a principal axis.

Apparently however this is not possible, except for rotation about a principal axis; and supposing this the axis of z, we must put $\Omega_1 = 0$, $\Omega_2 = 0$, $\xi = 0$, $\eta = 0$; and then we come to the cases considered on p. 245, *Proc. Cam. Phil. Soc.* Vol. III., supposing $\Omega_3 = \omega$ and $\zeta = \omega'$.

Hyper-elliptic functions are required for the general solution of equations (D) and (G); but if the ellipsoid be of revolution, the solution can be given by elliptic functions.

For if $a = b$, the third equations of (D) and (G) both lead to

$$\frac{d\zeta}{dt} = \frac{2c^2}{a^2 + c^2}(\Omega_2\xi - \Omega_1\eta).$$

We may put $\Omega_3 = 0$, and then from (G) and (D)

$$\frac{d\xi}{dt} = -\frac{2a^2}{a^2 + c^2}\Omega_2\zeta,$$

$$\frac{d\eta}{dt} = \frac{2a^2}{a^2 + c^2}\Omega_1\zeta,$$

$$\frac{d\Omega_1}{dt} = \Omega_2\zeta - \frac{a^2 + c^2}{a^2 - c^2}\eta\zeta,$$

$$\frac{d\Omega_2}{dt} = -\Omega_1\zeta - \frac{a^2 + c^2}{a^2 - c^2}\zeta\xi.$$

Three integrals of these equations are

$$\xi^2 + \eta^2 = L - \frac{a^2}{c^2}\zeta^2,$$

$$\Omega_1^2 + \Omega_2^2 = M + \frac{a^2 - c^2}{2c^2}\zeta^2,$$

$$\Omega_1\xi + \Omega_2\eta = N + \frac{a^2 + c^2}{4c^2}\zeta^2,$$

here L, M, N are constants.

Therefore

$$\left(\frac{d\zeta}{dt}\right)^2 = \frac{4c^4}{(a^2+c^2)^2}(\Omega_2\xi - \Omega_1\eta)^2$$

$$= \frac{4c^4}{(a^2+c^2)^2}\{(\Omega_1^2 + \Omega_2)(\xi^2+\eta^2) - (\Omega_1\xi + \Omega_2\eta)^2\}$$

$$= \frac{4c^4}{(a^2+c^2)^2}\left\{\left(L - \frac{a^2}{c^2}\zeta^2\right)\left(M + \frac{a^2-c^2}{2c^2}\zeta^2\right) - \left(N + \frac{a^2+c^2}{4c^2}\zeta^2\right)^2\right\}$$

$$= \frac{4c^4}{(a^2+c^2)^2}\left\{LM - N^2 + \left(L\frac{a^2-c^2}{2c^2} - M\frac{a^2}{c^2} - N\frac{a^2+c^2}{2c^2}\right)\zeta^2 - \left(\frac{3a^2-c^2}{4c^2}\right)^2\zeta^4\right\},$$

nd therefore ζ is an elliptic function, either the cn or dn, of the me t. But if $c^2 = 3a^2$, then ζ is a trigonometrical function of t.

Again, put $\quad \Omega_1 = \Omega\cos\phi, \quad \Omega_2 = -\Omega_1\sin\phi,$

that

$$\Omega^2 = M + \frac{a^2-c^2}{2c^2}\zeta^2;$$

hen

$$\frac{d\Omega_1}{dt}\Omega_2 - \Omega_1\frac{d\Omega_2}{dt} = \Omega^2\frac{d\phi}{dt}$$

$$= (\Omega_1^2 + \Omega_2^2)\zeta - \frac{a^2+c^2}{a^2-c^2}(\Omega_1\xi + \Omega_2\eta)\zeta,$$

r

$$\frac{d\phi}{dt} = \zeta - \frac{a^2+c^2}{a^2-c^2}\frac{N + \dfrac{a^2+c^2}{4c^2}\zeta^2}{M + \dfrac{a^2-c^2}{2c^2}\zeta^2}.\zeta, \quad \ldots\ldots\ldots\ldots\ldots (K),$$

and therefore ϕ will be expressed by elliptic integrals of the third kind.

Put
$$\xi = \omega \cos\psi, \quad \eta = -\omega \sin\psi,$$
then
$$\omega^2 = L - \frac{a^2}{c^2}\zeta^2,$$
also
$$\omega\Omega\cos(\phi - \psi) = N + \frac{a^2+c^2}{2c^2}\zeta^2;$$
and
$$\frac{d\xi}{dt}\eta - \xi\frac{d\eta}{dt} = \omega^2\frac{d\psi}{dt}$$
$$= -\frac{2a^2}{a^2+c^2}(\Omega_1\xi + \Omega_2\eta)\,\zeta,$$
or
$$\frac{d\psi}{dt} = -\frac{2a^2}{a^2+c^2}\,\frac{N + \dfrac{a^2+c^2}{4c^2}\zeta^2}{L - \dfrac{a^2}{c^2}\zeta^2}\,\zeta,$$

and therefore ψ will also be expressed by elliptic integrals of the third kind.

In a state of steady motion, ζ is constant, and therefore
$$\frac{d\zeta}{dt} = 0,$$
or
$$\frac{\Omega_1}{\xi} = \frac{\Omega_2}{\eta}.$$

Also $\dfrac{d\phi}{dt}$ and $\dfrac{d\psi}{dt}$ are constant, and equal to n suppose.

Therefore if
$$\Omega_1 = \Omega\cos nt, \quad \Omega_2 = -\Omega\sin nt,$$
then
$$\xi = -\frac{2a^2}{a^2+c^2}\frac{\zeta}{n}\Omega\cos nt, \quad \eta = \frac{2a^2}{a^2+c^2}\frac{\zeta}{n}\Omega\sin nt,$$
so that
$$\omega = -\frac{2a^2}{a^2+c^2}\frac{\zeta}{n}\Omega.$$

Then the equation (K) leads to
$$n^2 - n\zeta - \frac{2a^2}{a^2-c^2}\zeta^2 = 0,$$

or
$$(n-\tfrac{1}{2}\zeta)^2=\frac{9a^2-c^2}{a^2-c^2}\zeta^2;$$

and therefore a state of steady motion is not possible if
$$3a>c>a.$$

Or, otherwise, since $\phi=\psi$,
$$\Omega_1\xi+\Omega_2\eta=\Omega\omega;$$

therefore
$$\frac{d\psi}{dt}=-\frac{2a^2}{a^2+c^2}\frac{\Omega}{\omega}\zeta,$$

and
$$\frac{d\phi}{dt}=\zeta-\frac{a^2+c^2}{a^2-c^2}\frac{\omega}{\Omega}\zeta;$$

and since
$$\frac{d\phi}{dt}=\frac{d\psi}{dt},$$

therefore
$$\frac{2a^2}{a^2+c^2}\frac{\Omega}{\omega}+1-\frac{a^2+c^2}{a^2-c^2}\frac{\omega}{\Omega}=0,$$

or
$$\left(\frac{\omega}{\Omega}-\frac{1}{2}\frac{a^2-c^2}{a^2+c^2}\right)^2=\frac{(a^2-c^2)(9a^2-c^2)}{4(a^2+c^2)^2};$$

and therefore $9a^2>c^2>a^2$ for the roots of this quadratic in $\omega:\Omega$ to be imaginary, and therefore a state of steady motion impossible.

Mr H. W. G. Mackenzie has pointed out to me a very simple way of reducing the hydrodynamical equations to the form
$$\frac{1}{\rho}\frac{dp}{dx}+A'x=0,\quad \frac{1}{\rho}\frac{dp}{dy}+B'y=0,\quad \frac{1}{\rho}\frac{dp}{dz}+C'z=0.$$

For the hydrodynamical equations are of the form
$$\frac{1}{\rho}\frac{dp}{dx}+Ax+\alpha x+hy+gz=0,$$
$$\frac{1}{\rho}\frac{dp}{dy}+By+hx+\beta y+fz=0,$$
$$\frac{1}{\rho}\frac{dp}{dz}+Cz+gx+fy+\gamma z=0;$$

and we see that the component accelerations in space of the liquid particle at xyz parallel to the co-ordinate axes are respectively

$$ax + hy + gz,$$
$$hx + \beta y + fz,$$
$$gx + fy + \gamma z;$$

and by the dynamical equations, the rates of change of angula momentum about the co-ordinate axes are zero, and therefore

$$\Sigma m \{(gx + fy + \gamma z)\, y - (hx + \beta y + fz)\, z\} = 0,$$

or
$$f \Sigma m\, (y^2 - z^2) = 0,$$

or
$$f(b^2 - c^2) = 0.$$

Therefore $f = 0$, and similarly g and h vanish.

Therefore the hydrodynamical equations reduce to

$$\frac{1}{\rho}\frac{dp}{dx} + Ax + \alpha x = 0,$$
$$\frac{1}{\rho}\frac{dp}{dy} + By + \beta y = 0,$$
$$\frac{1}{\rho}\frac{dp}{dz} + Cz + \gamma z = 0.$$

For experimental illustrations see a paper in *Nature*, Nov. 1 1880, by Sir W. Thomson, "On an experimental illustration o Minimum Energy."

(2) *On the history of geometrical continuity.* By C. TAYLO M.A., Fellow of St John's College.

The foci of the ellipse and the hyperbola were known t Apollonius of Perga in the third century B.C., and in all proba bility to none before him; since in the first place there is n earlier trace of them, and in the next place they are introduce in the third book of his *Conica*, of which he remarks that it con tains many wonderful theorems, for the most part new. H determined the foci by a process of "application" (παραβολή) areas, which amounted to dividing the transverse axis into pai of segments whose product is equal to the square of the conjugat semi-axis.

It is a fundamental fact in the history of continuity tha Apollonius failed to discover any focus of the parabola, the are to be "applied" and the axis to which it was to be applied bein

n this case infinite. For the earliest trace of a focus of the parabola we refer to proposition 238 of the seventh book of the *Collectio* of Pappus (p. 1013 ed. Hultsch), where the property of the focus, directrix, and determining ratio is given; but still the difficulty which presented itself to Apollonius is not in any direct way surmounted.

The foci long continued to be spoken of as the points arising from the "application," *puncta ex applicatione facta*, with reference to the above-mentioned construction of Apollonius. At a later period they were called *umbilici*, *foci*, and occasionally *poles*. Some time back I was engaged in an attempt to trace the origin of the name "focus" of a conic, not finding any correct information about its earliest use in the historical works with which I was acquainted. At length I lighted upon a work in which it was written, that the points in question, although sufficiently defined by their properties, had *nomen nullum*, and the name *foci* was accordingly proposed, with reference to their optical or reflexional property in relation to the conic. The writer was KEPLER. I had thus come to the end of my investigation, and not only so, but had found much more than I was then in search of; for in the same passage in which he gives to the points described by the periphrasis *puncta ex applicatione facta* their new name of Foci, he clearly and decisively lays down the law of Continuity, the vital principle of the modern geometry.

The work of Kepler entitled *Ad Vitellionem* paralipomena quibus Astronomiæ pars Optica traditur* (Francofurti, 1604) contains a short discussion *De Coni Sectionibus* (cap. IV. § 4, pp. 92—6) from the point of view of analogy or continuity. The section of a cone by a plane "aut est Recta, aut Circulus, aut Parabole aut Hyperbole aut Ellipsis." Of all hyperbolas "obtusissima est linea recta, acutissima parabole;" and of all ellipses "acutissima est parabole, obtusissima circulus." The parabola is thus intermediate in its nature to the hyperbola and "recta" (or line pair) on the one hand, and the closed curves the ellipse and the circle on the other; "infinita enim & ipsa est, sed finitionem ex altera parte affectat." He then goes on to speak of certain points related to the sections, "quæ definitionem certam habent, *nomen nullum*, nisi pro nomine definitionem aut proprietatem aliquam usurpes." The lines from these points to any point on the curve make equal angles with the tangent thereat: "Nos lucis causa & oculis in Mechanicam intentis ea puncta FOCOS appellabimus." He would have called them *centres* if that term had not been already appropriated. In the circle there is one focus, coincident with the

* *Opticæ Thesaurus.* ALHAZENI *Arabis libri* VII. *Item* VITELLIONIS *libri* X. (Basil. 1572).

centre; in the ellipse or hyperbola two, equidistant from th centre: in the parabola one within the section, "*alter vel extra ve intra sectionem in axe fingendus est infinito intervallo a prior remotus, adeo ut educta HG vel IG* ex illo cæco foco in quodcun que punctum sectionis G sit axi DK parallelos.*"

In the circle the focus recedes as far as possible from th nearest part of the circumference, in the ellipse somewhat less, i the parabola much less; whilst in the line-pair the "focus," as h still calls it to complete the analogy, falls upon the line itsel Thus in the two extreme cases of the circle and the line-pai the two foci coincide. He then goes on to compare the latu rectum and its intercept on the axis, or as he calls them the *chord* and *sagitta,* in the several sections, concluding with the case of th line-pair, in which the chord *coincides with its arc,* "abusive si dicto, cum recta linea sit." But our geometrical expressions must b subject to analogy, "*plurimum namque amo analogias, fidelissimo meos magistros, omnium naturæ arcanorum conscios.*" And especia regard is to be had to these analogies in geometry, since they com prise, in however paradoxical terms, an infinity of cases lyin between opposite extremes, "totamque rei alicujus essentiam lucu lenter ponunt ob oculos."

(1) Hereupon be it remarked, that the principle of Analog on which he insists so fervently is the archetype of the principl of Continuity. The one term expresses the inner resemblance o contrasted figures *A* and *B*, which are connected by innumerabl intermediate forms; whilst the other expresses the possibility o passing through those intermediate forms from *A* to *B*, withou any change *per saltum.* Geometry was *not* indebted to Algebr for the suggestion of the law of continuity.

(2) Having traced the transition from the line-pair to th circle through the three standard forms of conics, he completes th theory of the points henceforth named Foci by the discovery o the "cæcus focus" of the parabola, which is to be taken at infinit on the axis either *without* or *within* the curve. The parabola ma therefore be regarded indifferently as a hyperbola, having (rela tively to either of its branches) one external and one interna focus, or as an ellipse, having both foci within the curve.

(3) The further focus of the parabola being taken at infinit on the axis in either direction, the two opposite extremities o every infinite straight line are thus regarded as coincident or con secutive points—a conception which may be shewn to conduc logically to the idea of imaginary points.

* The figure indicates that the line from the further focus may be considered t lie either within or without the parabola.

(4) Every straight line from the "cæcus focus" of the para-la to a point on the curve being said to be parallel to the axis, e idea of the concurrence of parallel lines at a point at infinity s at length been formed and announced. It is to be noticed at the new doctrine of parallels is here presented in relation to e plane, and not as springing out of the consideration of figures perspective in space.

Taking into account also Kepler's *Nova Stereometria* we con-de that by his contributions to the doctrine of the infinite and e infinitesimal and his firm grasp of the principle of continuity, is entitled to the foremost rank amongst the founders of the dern geometry.

November 8, 1880.

PROFESSOR NEWTON, PRESIDENT, IN THE CHAIR.

The following communications were made to the Society:

(1) *On a new arrangement for sensitive flames.* By Lord RAY-IGH, M.A., F.R.S., Professor of Experimental Physics.

A jet of coal gas from a pin-hole burner rises vertically in the erior of a cavity from which the air is excluded. It then sses into a brass tube a few inches long, and on reaching the , burns in the open. The front wall of the cavity is formed of exible membrane of tissue paper, through which external sounds reach the burner.

The principle is the same as that of Barry's flame described by ndall. In both cases the *unignited* part of the jet is the sen-ive agent, and the flame is only an indicator. Barry's flame y be made very sensitive to sound, but it is open to the objec-n of liability to disturbance by the slightest draught. A few ars since Mr Ridout proposed to enclose the jet in a tube air-ht at the bottom, and to ignite it only on arrival at the top of s tube. In this case however external vibrations have very perfect access to the sensitive part of the jet, and when they ch it they are of the wrong quality, having but little motion nsverse to the direction of the jet. The arrangement now ex-ited combines very satisfactorily sensitiveness to sound, and ensitiveness to wind, and it requires no higher pressure than t of ordinary gas-pipes. If the extreme of sensitiveness be ned at, the gas pressure must be adjusted until the jet is on the int of flaring without sound.

The apparatus exhibited was made in Prof. Stuart's worksho An adjustment for directing the jet exactly up the middle of th brass tube is found necessary, and some advantage is gained b contracting the tube somewhat at the place of ignition.

(2) *On an effect of vibrations upon a suspended disc.* By Lo RAYLEIGH, M.A., F.R.S.

In the British Association experiment for determining the un of electrical resistance, a magnet and mirror are enclosed in wooden box, attached to the lower end of a tube through whic the silk suspension fibre passes. Under these circumstances it found that the slightest tap with the finger-nail upon the bo deflects the mirror to an extraordinary degree. The disturban appears to be due to aerial vibrations within the box, acting upo the mirror. We know that a flat body, like a mirror, tends to s itself across the direction of any steady current of the fluid which it is immersed, and we may fairly suppose that an effect the same character will follow from an alternating current. A the moment of the tap upon the box the air inside is made to mov past the mirror, and probably executes several vibrations. Whi these vibrations last, the mirror is subject to a twisting force tend ing to set it at right angles to the direction of the vibration. Th whole action being over in a time very small compared with tha of the free vibrations of the magnet and mirror, the observe effect is as if an impulse had been given to the suspended parts.

The experiment shewn is intended to illustrate this effect. small disc of paper, about the size of a sixpence, is hung by a fin silk fibre across the mouth of a resonator of pitch 128. When sound of this pitch is excited, there is a powerful rush of air in an out of the resonator, and the disc sets itself promptly across the pa sage. A fork of pitch 128 may be held near the resonator, but it better to use a second resonator at a little distance in order t avoid any possible disturbance due to the neighbourhood of th vibrating prongs.

(3) *On an apparatus illustrating the movement of sound-wav and water-waves.* By SEDLEY TAYLOR, M.A., late Fellow of Trinit College.

This was an apparatus illustrating wave-motion. The arrange ment consists of sixteen toothed wheels of brass, centred along straight line upon a flat board, and connected by intermediat pinions in such a manner that, when one of the end-wheels is set i motion by means of a winch attached to it, all the others rotate wit the same velocity and in the same direction. Sixteen slender stem

equal lengths, each supporting a small wooden ball, are inserted the wheels perpendicular to their plane near their circumferences, such points that the phase-difference between any two adjacent lls is one-eighth of a revolution. The balls thus disposed reprent two complete equal waves, and, when the winch is turned, e effect of wave-motion is produced. Different positions of an server's eye with reference to the apparatus lead to the presentan of waves due to different types of particle-movement, both bital and vibrational. Let the board be first placed with its ane vertical, and with the straight line joining the centres of the eeels horizontal. Let the observer's eye be situated in front of, d at some distance from, the apparatus, in a horizontal plane rough the line of centres. Waves due to circular particle-moveents in the plane of wave-propagation, like those on the surface deep water, are now seen. The observer's eye remaining stanary, let the board be next gradually turned about the line of ntres. The circles are thus projected into ellipses with major es horizontal, such as give rise to water-waves below the surce. These degenerate, when the plane of the board becomes rizontal, into straight vibrational paths in the direction of progation, harmonically described, and giving rise to waves like ose of a sound of one degree of pitch, or 'simple tone.' By itably altering the position of the observer's eye in the plane on hich it is situated, these paths can be made to look as if executed liquely to the direction of wave-propagation. Lastly, replacing e board in its original position, let it be gradually turned about vertical axis on its own plane. The circles then pass into ellipses th vertical major axes, degenerating ultimately into straight es transverse to the direction of wave-propagation, and thus esenting the case of plane-polarized light. All the types of ave-motion ordinarily requiring illustration, with the exception circularly and elliptically polarized light, are, therefore, capable being represented by this apparatus.

November 22, 1880.

PROFESSOR NEWTON, PRESIDENT, IN THE CHAIR.

C. Creighton, M.A., Demonstrator of Anatomy, was balloted r and duly elected a Fellow of the Society.

The following communications were made to the Society:

(1) *On the experiments made by Biot and others on horizontal fraction.* By J. B. PEARSON, D.D., Fellow of Emmanuel College.

I was led to investigate this question by some remarks made

upon a memoir I presented to the Society last spring (*Proceedings*, Vol. III. pp. 352—8), describing some observations of the Sun on the northern horizon, made by me last summer at the North Cape. At the commencement of this century, the question seems to have excited some interest: there being papers on the subject by Mr Huddart, Prof. Vince, and Mr Wollaston in the *Philosophical Transactions* for 1797, 1799, and 1803. As these memoirs however do not seem to me to contain any precise results, although they record several interesting natural phenomena, I shall not trouble my readers with an abstract of them. The investigations however, of M. Biot, M. Le Gentil, and in a less degree those of M. Bouguer in Peru, seem to me so relevant to the question of my own observations, that I have thought that an abstract of them may be of interest to science.

It must be premised that the altitude of the Sun or a star when close to the sea-horizon involves two things: the apparent depression of the horizon itself, or the dip, technically so called, and the atmospheric refraction. M. Biot's experiments are confined to the former; those of the two other *savans* mainly to the latter: while my own observations, taken on the sea-horizon from the deck of a ship at a height of about 18 feet, will evidently be affected by both.

Having premised this, I subjoin first an abstract of M. Biot's observations, which were taken by him with the aid of M. Mathieu at Dunkirk during the winter of 1809—10, and were published by him at Paris in a separate volume in the following year. They were taken with a graduated quarter-circle with telescope attached and adjusted by a spirit level, and from stations at different altitudes above the level of the sea: viz. (1) actually on the sand: (2) a timber staging: (3) different stories of a house on the beach: and lastly, from the top of the tower of the church. The annexed Table shews the place of observation, and its elevation in feet, the date, the temperatures of the sea and air on Fahrenheit's scale, and the amount by which the observed result varied from that which would be expected on theoretical grounds, a negative sign (−) being affixed where the depression of the horizon was *greater* than the normal, and a positive one (+) when it is *less*. By the normal or theoretical result is meant that given by the theory explained at length by M. Biot in the same volume. I am unable to say how far his theory at that time coincides with the views on the subject laid by him before the French Academy in several papers in the years 1854 and 1855. He then allowed that the system generally adopted was that of Bessel, and spoke with approval of the abstract of it issued by the Greenwich Observatory in 1853: but he was evidently dissatisfied with Bessel's method, as

ll as with Ivory's, at any rate for low altitudes; preferring to ploy a formula of Laplace's, corrected by experiments made der all different circumstances. Myself, merely for convenience, employ Ivory's Tables, which at Z. D. 89°. 30″, Th. 50 F. Bar. ·6, differ from Bessel's by 33″ only: but I subjoin an extract from Biot's concluding paper, as probably of value on a subject of ich I have little or no theoretical knowledge myself.

M. Biot in summing up the results of the series of papers pubhed by him in 1854, 5, seems to say that he looks with great spicion on any but the simplest theory of refraction, at any rate low altitudes.

He says that he has taken as a basis of his arguments the mula of Laplace, which he seems entirely to accept: but en he continues by saying, that when he constructs atmospheres ch as the theories of Ivory and Bessel assume......"en comparant s résultats à ce que nous connaissons de l'atmosphère reelle, on erçoit avec évidence, qu'aucune de ces atmosphères hypothéties ne lui est, même approximativement, assimilable; et qu'ainsi es ne peuvent pas donner les vraies réfractions; surtout celles i s'opérant près de l'horizon, se montrent perpétuellement oublées par des accidents lointains, dont les hypothèses ne tiennt aucun compte. A cela on pourra répondre que ces dernières fractions échappent inévitablement à toute théorie; et que, dans npossibilité où l'on est de prévoir leurs caprices, on ne doit mander aux hypothéses que de reproduire leur valeurs moyennes. est en effet un des genres d'utilité qu'Ivory et Bessel ont étendu obtenir de celles qu'ils ont employées. Mais alors il udrait, comme l'a fait Laplace, borner l'empirisme à cette portion égulière du phénomène, et ne pas l'étendre à des déterminations i peuvent en être rendues indépendantes. Même pour ce but rticulier, les hypothèses sont encore inutiles. Car en s'aidant de formule de Laplace judicieusement appliquée, on peut, comme je montre, obtenir par observation seule, de Tables de ces valeurs oyennes qui seront propres à chaque localité; qui les donneront lles qu'elles se produisent réellement,...et qui offriront encore t avantage, que si, un peu au delà des distances zénithales auxelles la formule de Laplace s'applique, il existe entre les réfracons et les indications météorologiques quelque relation assez nstante pour qu'on puisse s'en prévaloir, on aura toute chance de découvrir. Des Tables ainsi construites d'après l'observation re, pour les distances zénithales que la formule approximative peut atteindre, fourniraient sur la constitution des couches férieures de l'atmosphère, des documents certains, qui se rattacheient efficacement à ceux que les physiciens croient recueillir ns ces couches mêmes, ce qui aurait le double avantage d'assurer présent, et de préparer l'avenir." *C. R.* XL. 603, 4. (1855).

Table of Depressions of the Sea-horizon. 1808-9.

Place of Observation.	Elevation above Sea-level.	Dec. 26.	Jan. 16.	Feb. 5.	Feb. 6.	Feb. 8.	Feb. 10.	Feb. 14.
		B. 29·6. S. 32. A. 25.	B. 30·2. S. 32. A. 24.	B. 29·5. S. — A. 48.	B. 29·6. S. 44. A. 47.	B. 30·0. S. 39. A. 37.	B. 29·4. S. 42. A. 50.	B. 29·6. S. 46. A. 50.
1) On the Sand	$2\frac{1}{2}$	3′ 24″ (–)	14″ (–)	1′ 46″ (+)	1′ 11″ (+)	10″ (–)	3′ 0″ (+)	1′ 38″ (+)
2) ,, Quay	7	...	...	...	...	...	...	1′ 32″ (+)
3) ,, Platform	25	1′ 49″ (–)	...	...	...	...	...	...
4) ,, 1st story of house.	39	1′ 0″ (–)	1′ 30″ (–)	...	...	1′ 7″ (–)	1′ 31″ (+)	1′ 20″ (+)
5) ,, 3rd story of house.	61	26″ (–)	1′ 11″ (–)	...	...	21″ (–)	1′ 43″ (+)	1′ 20″ (+)
6) ,, Terrace of house ..	70	...	42″ (–)	...	...	...	...	1′ 21″ (+)
7) ,, Tower of church..	204	...	43″ (+)	...	...	...	2′ 14″ (+)	...

N.B.—The elevation is given in English feet, and is the *mean* elevation, the *true* of course varying with the state of the tide. B. signifies the height of the barometer in English inches; S. the temperature (Fah.) of the sea; A. that of the air. The observations of Feb. 5 and 6 are the mean result of a considerable series, which I need not reprint here in detail. They will be found at pages 167, 8 of Biot's work.

I may as well insert here one observation of my own, of a con-ct of the Sun's lower limb with the sea-horizon, which I timed curately, at Calais, on the sands west of the pier, with my eye at elevation of about 18 feet above the level of the water, on the ening of Sept. 18, 1877, at about 5h. 56m. 40s. G. M. T. The sea ing calm, and the horizon and sky perfectly clear, I consider it rth appending to the observations of which I have given an stract.

Assuming Ivory's theory of refraction, which can be employed greater zenith-distances than Bessel's, to be approximately rrect, I find the zenith distance of the Sun's centre, assuming e usual allowance of 4′ 20″ for dip, to have been when its lower nb touched the horizon 90° 23′ 27″: whereas, calculating its ace by theory, it ought to have been 90° 24′ 27″, or 1′ greater. is would seem to shew that at the moment when I observed the n, the depression of the visible sea-horizon, was that much eater than the normal, or 5′ 20″ about. Now at Dunkirk, in e first story of the house, where the observer's elevation was uch the same, viz. 28—35 feet, when the sea was warmer an the air the dip was greater than the normal, (1) 1′. 49″,) 1′ 30″, (3) 1′ 38″, (4) 37″; but when the sea was colder than e air the dip, from the same position, was less, (1) 1′ 20″,) 1′ 25″, (3) 1′ 37″, (4) 1′ 25″. When I saw the Sun setting, e evening chill was rapidly coming on, and so the temperature of e air probably falling faster than, if not below, that of the sea: e result thus agreeing in a certain degree with that observed at ınkirk. I had no thermometer to note the temperature of the a water, but that of the air, as well as the height of the baro-eter, I got on the pier.

It results clearly from these observations—I mean of course ainly those of Biot, that the temperature of the atmosphere can a very important factor in depressing or elevating the visible a-horizon, and probably also the observed altitude of any distant ject, with reference to the true horizon at the point of observa-on. In observing from the higher points, allowance was made r the curvature of the earth, as is stated by Biot at p. 151 of s work.

I will now proceed to consider the effect of refraction of the sition of the Sun on or near the sea-horizon, as observed by Gentil at Pondicherry in 1769. His observations will be found ven in considerable detail in the *Mémoires de l'Academie des iences*, 1774, pp. 330—350.

Le Gentil prefaces his remarks by saying that at the beginning of e 18th century there was an impression that refractions at the Pole ere much greater (perhaps double) what they are at the Equator,

and mentions that Maupertuis had dissented from this view, adding that the well-known asserted sight of the Sun by the Dutch in Nova Zembla, on Jan. 24, 1597, cannot be relied on as an element to be taken into account in the discussion.

He then states that when at Port Louis (20° 10′ S. Lat.), in the Isle de France or Mauritius, he noticed in winter that the Sun never set behind the visible sea-horizon, but a false horizon, four to five minutes (arc) higher. This observation he could not verify in summer, the sea-horizon being then interrupted by land; and at Manilla, where he went to observe the transit of Venus in 1761, a suitable place for similar observations could not be obtained, but when he had established himself at Pondicherry, on the Coromandel coast, he soon found that the peculiarity which he had noticed at Port Louis entirely repeated itself: the Sun invariably rising in winter, not from behind the sea-horizon, but from as it were a bank of mist about 5′ higher, and occupying only about 35 seconds of time in reaching the horizontal wire of his telescope when adjusted at zero: whereas in summer, it rose always from behind the natural sea-horizon, and occupied nearly a minute, 59 seconds on one occasion, in reaching the horizontal wire of the telescope, and with this difference in brightness, that the darkened glass was required in summer as soon as the Sun's upper limb had passed the horizon, while in winter it could be viewed with the naked eye up to one degree of elevation. He ascribes this phenomena to the condensation of the atmosphere, even within the tropics during winter.

He then gives his actual observations, very fully recorded, of which I have analyzed and computed such as serve my purpose, as follows:

He commences by ascertaining the depression of the sea-horizon, which on Jan. 7, 1769, he found to be 10′ 50″ at an altitude of about 40 feet, or, allowing for an error of 1′ 58″ in the graduation of his instrument, a quarter circle with telescope apparently similar to that used by Biot, about 8′ 52″. As the normal value would be about 6′ 36″, it seems that the horizon appeared depressed about 2′ 16″ more than would be expected, and he says that he had the same result from an observation on the following day; and apparently also, during the summer, though he gives no more actual figures.

He then gives the actual moments of the Sun reaching the wire of his telescope, of course at a Zenith distance of 89° 58′ 2″, for a number of days during January and February, and also June and July. I have computed these, with the aid of the Nautical Almanac for 1769, employing Ivory's tables for refraction, and assuming the temperature at sunrise to have been 60° F. in winter

d 70°. F. in summer, Le Gentil merely saying that it did not ry much at the two seasons. It should be noticed that he gives the parent time with great accuracy, as ascertained by the method of qual Altitudes; and I have found that these Time observations rify themselves to considerable nicety, so that the times he gives ay be taken as known with comparative certainty, assuming the ong. and Lat. of Pondicherry, as given now in the *Connaisnce des Temps*, viz. 11° 55′ 40″ N. Lat. 5h. 9m. 57s. E. Long. of ris.

Working on this method, I obtain the following table of comrison between the true or reduced Zenith distance of the Sun's ntre, as obtained by the observations, and that given by the Sun's eoretical place at the same moment:

Winter, 1769.

Sun's centre.	Jan. 10.	Jan. 27.	Feb. 4.
Observed Z. D...	90° 47′ 37″	90° 47′ 35″	90° 47′ 34″
Theoretical Z. D.	90° 46′ 21″	90° 51′ 50″	90° 44′ 24″
	(−) 1′ 16″	(+) 4′ 15″	(−) 3′ 10″

Summer, 1769.

Sun's centre.	June 18.	July 14.	July 19.
Observed Z. D...	90° 46′ 25″	90° 46′ 25″	90° 46′ 26″
Theoretical Z. D.	90° 43′ 22″	90° 46′ 21″	90° 45′ 53″
	(+) 3′ 3″	(+) 4″	(+) 33″
Sun's centre.	July 20.	July 23.	July 27.
Observed Z. D...	90° 46′ 26″	90° 46′ 26″	90° 46′ 26″
Theoretical Z. D.	90° 46′ 34″	90° 44′ 42″	90° 46′ 12″
	(−) 8″	(+) 1′ 44″	(+) 14″

In all these observations there can be no change ascribed to e apparent sea-horizon, because the point of observation was e moment of the Sun's passing the wire of the telescope, which inted to a known Zenith distance. Refraction alone therefore

can here come into account. I have marked with a positive sign (+) the cases where the refraction must have been greater than the normal, and with a negative (–), when it must have been less. By the *normal*, I mean that obtained by Ivory's Tables, which seem to me very convenient for very low altitudes, and, as I have mentioned already, at an altitude of half a degree only vary from those of Bessel by 33 seconds.

Next I will give the *error*, as I may call it, of the Sun's altitude at the moment of its emerging from behind the horizon, *false* or *true*, in summer or in winter: that is the amount by which the measured Zenith distance differs from that suggested by theory, a discrepancy which, as will be seen from Le Gentil's description of his work, must mainly be due to refraction, but which also may have been slightly due to variations in the depression of the visible sea-horizon, though the latter seems always at Pondicherry to have been greater than would have been expected by theory; the *dip* given in the books for 40 (Paris) feet, being about 6′ 25″: whereas he says that his instrument always made it about 8′ 50″. I may observe, in passing, that with a theodolite, I found the same result at Vardö in Finmark; so far, that the dip given by the instrument was always greater than my elevation required, though from various circumstances I could not determine the latter, about 30 feet, with accuracy.

Winter, 1769.

Sun's centre.	Jan. 7.	Jan. 9.	Jan. 10.	Feb. 1.
Observed Z. D...	90° 55′ 34″	90° 55′ 34″	90° 55′ 34″	90° 55′ 30″
Theoretical Z. D.	90° 53′ 40″	90° 54′ 25″	90° 54′ 17″	90° 53′ 40″
	(+) 1′ 54″	(+) 1′ 9″	(+) 1′ 17″	(+) 1′ 50″

Summer, 1769.

Sun's centre.	July 14.	July 26.
Observed Z. D. ...	90° 59′ 43″	90° 59′ 44″
Theoretical Z. D..	90° 59′ 57″	91° 2′ 1″
	(–) 16″	(–) 2′ 17″

N.B.—As already mentioned, the sign (+) signifies that the refraction is greater than would be expected, (–) that it is less.

Comparing Le Gentil's computed results, the method of obtaining which he does not explain very clearly, we get

Horizontal Refraction.

1769.	Le Gentil.	Self.
Jan. 10	28′ 9″	32′ 8″
,, 11	27′ 57″	...
,, 28	25′ 10″	37′ 39″
Feb. 4	27′ 27″	30′ 10″
	4) 108′ 43″	3) 99′ 57″
	27′ 11″	33′ 19″
June 18	24′ 15″	29′ 41″
July 14	25′ 36″	32′ 40″
,, 19	27′ 0″	33′ 16″
,, 20	29′ 24″	32′ 38″
,, 23	27′ 26″	30′ 59″
,, 27	29′ 14″	32′ 31″
	6) 162′ 55″	191′ 42″
	27′ 9″	31′ 57″

ue Zenith Distance of the Sun's upper limb when first visible.

	Le Gentil.	Self.
Jan. 7	90° 35′ 35″	90° 37′ 30″
,, 9	,, 36′ 28″	,, 38′ 15″
,, 10	,, 36′ 6″	,, 38′ 7″
Feb. 1	,, 36′ 31″	,, 37′ 34″
Mean	90° 36′ 10″	90° 37′ 52″
July 14	90° 42′ 7″	90° 44′ 9″
,, 26	,, 44′ 52″	,, 46′ 14″
Mean	90° 43′ 30″	90° 45′ 12″

The discrepancy between the two columns, in the case of refraction, is I own very serious; but I cannot change my own figures, or see exactly how Le Gentil got his own. The normal refraction according to Bessel for Z. D. 89° 30′ is about 29′ 3″, with a probable error, from experiment, of ± 20″: Ivory making it 28′ 31″. The former gives nothing below this, but at 90° Z. D. according to Ivory, with the Bar. at 30°, as I have assumed throughout my calculations, and the Therm. at 60 F. in winter and 70 F. in summer, which seems reasonable, we get 33′ 50″ and 33′ 10″ respectively, Bessel's system probably giving in each case about 40″ more. My own mean results being 33′ 19″ and 32′ 7″: it would seem that the allowance for temperature made by Ivory is hardly sufficient, as Le Gentil says (p. 342) that the difference between the summer and winter temperature at Pondicherry was not more than 5° to 6° (R.) in winter in the morning and 13° to 14° in the afternoon. Any how I do not think the refraction can have been as small as Le Gentil makes it in his summary (p. 248): it is to be regretted that he kept so much in his mind the phenomenon said to have been seen by the Dutch in Nova Zembla: which cannot in strictness be thought more than interesting.

I shall conclude this paper with a brief résumé of the observations on horizontal refraction made by M. Bouguer in Peru; which are given and discussed by him in two articles in the *Mémoires* of 1739 and 1749. It should be noticed that he only records his actual results, and not his complete observations, or his methods of procedure.

Experiments in Peru.

M. Bouguer (9—23 April, 1736) found, as he asserts, the refraction for the setting Sun, on the sea-shore, in 1° 1′ S. Lat. to be 25′ to 29′... at 1° alt. 20½′... The modern tables giving, as the normal refraction, 33′ and 24½′ about, in these cases, respectively. He thus agrees more or less nearly with Le Gentil in his estimate of refraction generally.

On the sea-coast, on another occasion, at an alt. of 1°, but at an elevation of 40 toises (= 256 ft.), he found it 22′ 15″. At Quito, at an altitude of 1400 toises (= 8960 Eng. ft.), he says it was for 2° 20″ alt. 12′ 1″ (the normal being 17′), at 3° alt. 9′ 33″ (norm. 14′ 36″), at 4° alt. 8′ (norm. 11′ 51″).

On the height of Pichincha, near Quito (elevation 527 toises, = 3373 Eng. ft.), he was unsuccessful in obtaining any observations of value, for various reasons: but on Chimborazo, at an elevation of 2388 toises (= 15,283 Eng. ft.) he obtained the following results from the Sun:

Alt. of object.	Refraction.	No. of observations.	
1°	13′ 53″	3 (mean of)	Temperature not given.
0° 17′	18′ 9″	1	Barometer of course steady.
0	19′ 45″	3 (mean of)	
0° 31′ (–)	24′ 20″	1	
1° 0′ (–)	30′ 1″	1	
1° 7′ (–)	31′ 55″	2 (mean of)	
1° 17′ (–)	34′ 47″	1	

It is to be regretted that the observations on stars rising or setting near the prime vertical, made by Argelander at Konigsberg in 1820, 1; and referred to in the conclusion of the preface to the *Tabulæ Regiomontanæ* are not in print: though there is no reason to think that they would furnish anything exceptional. I know of no others that would be of value in determining whether under any circumstances horizontal refraction usually sinks to so low an amount as it seems to have done in the opinion of the French observers; or as there seems reason to think, from my own observations of last year, it will sometimes do under very peculiar circumstances. On that occasion it must be remembered that the rays of the Sun were passing through strata of atmosphere lying over a probably much colder portion of the sea than that near my own position. A horizontal line drawn at any point on the earth's surface will be at an elevation of 1000 yards at a distance of about 70 miles, and for 2000 yards at about 100 miles distance: but by the laws of refraction the track of any ray at either of these distances must have been much lower than this: and though I feel myself unable to suggest what its elevation at various distances would have been, I am sure that the distance would have been so great for no very high elevation as to bring it sensibly under the direct influence of the Polar Ice. And although the natural effect of cold is to increase refraction, the circumstances may have been sufficiently abnormal to produce the effects observed.

With these remarks, I feel justified in leaving the question with such of my readers as may feel interested in it.

(2) *On the problem of two pulsating spheres in a fluid* (Part II.). By W. M. Hicks, M.A., Fellow of St John's College.

In a paper read before the Society in the Michaelmas term of last year,* I shewed how the force between two pulsating spheres might be determined, by means of mass-images, and I calculated

* *Proceedings*, Vol. III. pp. 276—285.

the term depending on the time variation of the potential. By an oversight I treated the term depending on the square of the velocity as if it had no effect on the resultant force. This is only true if powers of the inverse distance higher than the second be neglected (as was the case in the numerical examples). It is of course not true in general, except in the case of the resultant-force on the system regarded as rigidly connected. In the present communication I propose to shew how the other terms may be found.

7. Using the same notation as in the former paper, the force on B from A is

$$X = \int p \cos\theta dS$$

$$= -2\pi b^2 \int_0^\pi \left\{\frac{\partial\phi}{\partial t} + \tfrac{1}{2} V^2\right\} \cos\theta \sin\theta \partial\theta.$$

To find the general term in the series for X directly by means of the infinite series for V would be extremely laborious, but this may be avoided by means of the following proposition.

Let ϕ be the velocity-potential of any motion symmetrical about a diameter of a sphere in the fluid, and such that the normal motion at the surface of the sphere is constant over the sphere, and equal to u. Then the force on the sphere is parallel to the diameter and is equal to

$$\pi \left\{b^2 \frac{\partial P}{\partial t} + buP - \tfrac{1}{2} Q + \tfrac{1}{4} b^2 \frac{\partial^2 Q}{\partial r^2}\right\},$$

where $$P = \int_0^\pi \phi \sin 2\theta d\theta,$$

$$Q = \int_0^\pi \phi^2 \sin 2\theta \partial\theta,$$

provided the velocity, along the surface, is always finite.

For consider the term $\pi b^2 \int_0^\pi V^2 \sin\theta \cos\theta \partial\theta$. At every point of the surface of the sphere $V^2 = u^2 + 1/b^2 (\partial\phi/\partial\theta)^2$. Since u^2 is constant the part of the force depending on it is zero, and the integral becomes

$$I = \frac{\pi}{2} \int_0^\pi \frac{\partial\phi}{\partial\theta} \sin 2\theta \,.\, \frac{\partial\phi}{\partial\theta} d\theta$$

$$=-\frac{\pi}{2}\int_0^\pi\left(\phi\frac{\partial^2\phi}{\partial\theta^2}\sin 2\theta+\frac{\partial\phi^2}{\partial\theta}\cos 2\theta\right)d\theta$$

$$=-\frac{\pi}{2}\int_0^\pi\phi\left(\frac{\partial^2\phi}{\partial\theta^2}+2\phi\right)\sin 2\theta\, d\theta-\frac{\pi}{2}\left[\phi^2\right]_0^\pi.$$

But by Laplace's equation

$$-\frac{\partial^2\phi}{\partial\theta^2}=2b\frac{\partial\phi}{\partial r}+b^2\frac{\partial^2\phi}{\partial r^2}+\cot\theta\frac{\partial\phi}{\partial\theta},$$

where in $\frac{\partial\phi}{\partial r}$, $\frac{\partial^2\phi}{\partial r^2}$ ... r is put $=b$, and therefore $\frac{\partial\phi}{\partial r}=u$; so that

$$I=\frac{\pi}{2}\int_0^\pi\phi\left\{\left(2bu+b^2\frac{\partial^2\phi}{\partial r^2}-2\phi\right)\sin 2\theta+2\cos^2\theta\frac{\partial\phi}{\partial\theta}\right\}d\theta-\frac{\pi}{2}\left[\phi^2\right]_0^\pi.$$

Now $$\int_0^\pi 2\cos^2\theta\,.\,\phi\frac{\partial\phi}{\partial\theta}d\theta=\int_0^\pi\phi^2\sin 2\theta\, d\theta+\left[\phi^2\right]_0^\pi=Q+\left[\phi^2\right]_0^\pi,$$

and $$\int_0^\pi\phi\frac{\partial^2\phi}{\partial r^2}\sin 2\theta\, d\theta=\int_0^\pi\left(\tfrac{1}{2}\frac{\partial^2\phi^2}{\partial r^2}-u^2\right)\sin 2\theta\, d\theta$$

$$=\tfrac{1}{2}\frac{\partial^2}{\partial r^2}\int_0^\pi\phi^2\sin 2\theta\, d\theta=\tfrac{1}{2}\frac{\partial^2 Q}{\partial r^2},$$

whence

$$I=\frac{\pi}{2}\left\{2buP-Q+\tfrac{1}{2}b^2\frac{\partial^2 Q}{\partial r^2}\right\},$$

and

$$X=-\pi\left\{b^2\frac{\partial P}{\partial t}+buP-\tfrac{1}{2}Q+\tfrac{1}{4}b^2\frac{\partial^2 Q}{\partial r^2}\right\},$$

when $r=b$.

8. We must now shew how P, Q may be determined when all the images are known. Still considering the general problem of symmetrical motion, it is known that ϕ can be expanded in a series of Zonal Harmonics

$$\phi=\Sigma\left\{A_i\left(\frac{r}{b}\right)^i+B_i\left(\frac{b}{r}\right)^{i+1}\right\}P_i$$

$$=\Sigma C_i P_i, \text{ say.}$$

The values of A_i, B_i being supposed known we have at once

$$P=2\,.\,\tfrac{2}{3}\,.\,C_1=\tfrac{4}{3}C_1.$$

Also $$Q = 2\int_{-1}^{+1} \{\Sigma C_i P_i\}^2 \mu d\mu.$$

Now P_i^2 remains unchanged when $-\mu$ is written for μ. Hence

$$\int_{-1}^{+1} P_i^2 \mu d\mu = 0,$$

and $$Q = 4\Sigma C_m C_n \int_{-1}^{+1} P_m P_n \mu d\mu,$$

the summation extending to all positive integral values of m, n, except $m = n$.

Let now $$\int_{\mu}^{1} P_m P_n d\mu = \Phi.$$

Then $$\int_{-1}^{+1} P_m P_n \mu d\mu = -\int_{-1}^{+1} P_m P_n d\mu + \int_{-1}^{+1} \Phi d\mu = \int_{-1}^{+1} \Phi d\mu.$$

Now (Ferrers's *Spherical Harmonics*, § 24),

$$\Phi = \frac{1}{(m-n)(m+n+1)} \left\{ \frac{n(n+1)}{2n+1} P_m (P_{n+1} - P_{n-1}) \right.$$
$$\left. - \frac{m(m+1)}{2m+1} P_n (P_{m+1} - P_{m-1}) \right\}.$$

Hence $\int_{-1}^{+1} \Phi d\mu$ vanishes unless $n = m+1$ or $m-1$. Its values in the two cases are

$$\frac{2(m+1)}{(2m+1)(2m+3)} \text{ and } \frac{2m}{(2m-1)(2m+1)}.$$

Since $\mu = P_1$ the value of the integral may also be found by substitution in Prof. Adams' formula for $\int_{-1}^{+1} P_m P_n P_p d\mu$. (*Proc. Roy. Soc.* XXVII. p. 71.)

Substituting the above values,

$$Q = 4\Sigma \left\{ \frac{2m}{(2m-1)(2m+1)} C_{m-1} C_m + \frac{2(m+1)}{(2m+1)(2m+3)} C_m C_{m+1} \right\}$$
$$= 16\Sigma_2 \frac{m}{(2m-1)(2m+1)} C_{m-1} C_m + \tfrac{8}{3} C_0 C_1.$$

[W]e require the value of $b^2 \dfrac{\partial^2 Q}{\partial r^2} - 2Q$, when $r = b$.

Now
$$C_m = A_m \left(\frac{r}{b}\right)^m + B_m \left(\frac{b}{r}\right)^{m+1};$$

[w]hence it may be shewn that

$$\left(b^2 \frac{\partial^2}{\partial r^2} - 2\right) C_{m-1} C_m$$

$$= 2m(2m-3) A_{m-1} A_m + 2m(2m+3) B_{m-1} B_m + 4A_{m-1} B_m - 2A_m B_{m-1},$$

[w]hich determines X in terms of the A, B.

9. It remains to shew how ϕ can be determined in a series of [Z]onal Harmonics. In doing this we will consider the fluid-motion [d]ue to any number of spheres, lying in a line, either at rest, or [p]ulsating, or moving along the line in any manner. This is a [sl]ightly more restricted case than is considered above, but the [m]ethod will apply to that as well.

Take any portion of an image within the sphere B under con[si]deration, say μ_n at a distance ρ_n from the centre of B. The [p]otential due to this is

$$= -\frac{\mu_n}{\rho_n} \left\{ \frac{\rho_n}{r} + \ldots + \left(\frac{\rho_n}{r}\right)^{i+1} P_i + \ldots \right\},$$

[th]erefore the part of B_i due to this is $-\mu_n \rho_n^{\,i}/b^{i+1}$ and

$$B_i = -\frac{1}{b^{i+1}} \Sigma (\mu_n \rho_n^{\,i}),$$

[th]e summation extending to all the sources and sinks within B. [S]o also

$$A_i = -b^i \Sigma \frac{\nu_n}{\sigma_n^{\,i+1}},$$

[th]e summation extending to all without B.

In the particular case we are considering, the sources and sinks [w]ill be arranged in systems of mass-images, each consisting of a [so]urce, and a line sink of constant density, and of magnitude [e]qual to the source. The part of B_i depending on the mass-image [w]hose source is μ_n is

$$-\frac{1}{b^{i+1}} \left\{ \mu_n \rho_n^{\,i} - \frac{\mu_n}{\rho_n - \rho_n'} \cdot \frac{\rho_n^{\,i+1} - \rho_n'^{\,i+1}}{i+1} \right\},$$

except for μ_0, from which the portion belonging to B_i is zero for every i except 0, and

$$B_0 = -\frac{\mu_0}{b},$$

$$B_i = -\frac{1}{b^{i+1}} \Sigma_1^\infty \mu_n \left\{\rho_n^{\,i} - \frac{1}{i+1} \frac{\rho_n^{\,i+1} - \rho_n'^{\,i+1}}{\rho_n - \rho_n'}\right\}.$$

In the same way is found

$$A_0 = -\frac{\nu_0}{c} - \Sigma_1^\infty \frac{\nu_n}{\sigma_n}\left\{1 - \frac{\sigma_n}{\sigma_n' - \sigma_n} \log\frac{\sigma_n'}{\sigma_n}\right\},$$

$$A_i = -\frac{b^i \nu_0}{c^{i+1}} - b^i \Sigma_1^\infty \frac{\nu_n}{\sigma_n^{\,i+1}}\left\{1 - \frac{1}{i}\frac{\sigma_n^{\,i+1}}{\sigma_n' - \sigma_n}\left(\frac{1}{\sigma_n^{\,i}} - \frac{1}{\sigma_n'^{\,i}}\right)\right\}.$$

Let A_i', A_i'', B_i', B_i'' be the parts of A_i, B_i depending on the pulsations of A, B respectively. Then μ_{n+1} is the image of ν_n and $\nu_0 = a^2 v$,

$$A_0' = -\frac{a^2 v}{c} - a^2 v\, \Sigma_1^\infty \frac{\nu_n'}{\nu_0}\left\{\frac{1}{\sigma_n} - \frac{1}{\sigma_n' - \sigma_n}\log\frac{\sigma_n'}{\sigma_n}\right\},$$

$$A_i' = -\frac{a^2 b^i v}{c^{i+1}} - a^2 b^i v\, \Sigma_1^\infty \frac{\nu_n'}{\nu_0}\left\{\frac{1}{\sigma_n^{\,i+1}} - \frac{1}{i\,(\sigma_n' - \sigma^n)}\left(\frac{1}{\sigma_n^{\,i}} - \frac{1}{\sigma_n'^{\,i}}\right)\right\},$$

$$B_0' = 0,$$

$$B_i' = -\frac{a^2 v}{b^{i+1}} \Sigma_1^\infty \frac{\mu_n'}{\nu_0}\left\{\rho_n^{\,i} - \frac{1}{i+1}\frac{\rho_n^{\,i+1} - \rho_n'^{\,i+1}}{\rho_n - \rho_n'}\right\}.$$

Also

$$A_0'' = -b^2 u \Sigma_1^\infty \frac{\nu_n''}{\mu_0}\left\{\frac{1}{\sigma_n} - \frac{1}{\sigma_n' - \sigma_n}\log\frac{\sigma_n'}{\sigma_n}\right\},$$

$$A_i'' = -b^{i+2} u \Sigma_1^\infty \frac{\nu_n''}{\mu_0}\left\{\frac{1}{\sigma_n^{\,i+1}} - \frac{1}{i\,(\sigma_n' - \sigma_n)}\left(\frac{1}{\sigma_n^{\,i}} - \frac{1}{\sigma_n'^{\,i}}\right)\right\},$$

$$B_0'' = -bu,$$

$$B_i'' = -\frac{u}{b^{i-1}} \Sigma_1^\infty \frac{\mu_n''}{\mu_0}\left\{\rho_n^{\,i} - \frac{1}{i+1}\frac{\rho_n^{\,i+1} - \rho_n'^{\,i+1}}{\rho_n - \rho_n'}\right\}.$$

By means of formulæ given in a paper in the *Transactions of the Royal Society*, 1880, Pt. II., p. 465, the values of ρ, σ, μ, ν can be expressed in terms of the radii of the spheres and their distance.

10. If as in the former paper we neglect that part of the ce which varies as a greater inverse power of the distance than e second, it is clear that we do not require to calculate higher ms than A_1, B_1; and to this degree of approximation it is easily und that, writing now r for the distance of the centres,

$$A_0 = -\frac{a^2 v}{r} - ab^2 u\left\{\frac{1}{r^2 - a^2} - \frac{1}{a^2}\log\frac{r^2}{r^2 - a^2}\right\},$$

$$A_1 = -\frac{a^2 bv}{r^2},$$

$$B_0 = -bu,$$

$$B_1 = -\frac{a^2 bv}{2r^2}.$$

Substituting, we easily find

$$\left(b^2\frac{\partial^2}{\partial r^2} - 2\right)C_0 C_1 = 2A_0 A_1 + 10 B_0 B_1 + 4A_0 B_1 - 2A_1 B_0$$

$$= 5\frac{a^2 b^2 uv}{r^2} - 2\frac{a^2 b^2 uv}{r^2} = 3\frac{a^2 b^2 uv}{r^2},$$

d the part of the force depending on Q is

$$-\frac{\pi}{4}\cdot\tfrac{8}{3}\cdot 3\frac{a^2 b^2 uv}{r^2} = -2\pi\frac{a^2 b^2 uv}{r^2}.$$

reover $$P = \tfrac{4}{3}(A_1 + B_1) = -2\frac{a^2 bv}{r^2};$$

erefore

$$\text{Force} = -\pi\left\{-2b^2\frac{\partial}{\partial t}\cdot\left(\frac{a^2 bv}{r^2}\right) - \frac{2a^2 b^2 uv}{r^2} + \frac{2a^2 b^2 uv}{r^2}\right\}$$

$$= \frac{2\pi b^2}{r^2}\frac{\partial}{\partial t}(a^2 bv),$$

 it is seen that the parts depending on V^2 have disappeared. the former paper the forces must be doubled throughout. s arises from an error on p. 280, l. 10, where 4π must be read 2π. In consequence of this on page 286 read $n\sqrt{2}$ for n and $0\sqrt{2}$ or ·014 for 1/50. When this correction is made the ression for the force agrees with the above.

(3) *On the death-struggle of a muscular fibre and the chemical physical changes which accompany it: together with an anation of the phenomena of shivering and rigor mortis.* By essor Latham.

(4) *On a sundial of a peculiar form, said to have been reconstructed by Lalande, at Bourg-en-Bresse, in France.* By Dr J. B. PEARSON, D.D., Fellow of Emmanuel College.

After the termination of the ordinary proceedings, a Special General Meeting, of which due notice in compliance with Section X. of the Bye-laws had been given, was held; at which it was moved by Professor Babington, and seconded by Professor Stokes, that for No. 2, Section VI. of the Bye-laws, the following be substituted:

"The President shall take the chair at 3 P.M. and shall quit it before 5 P.M."

The motion was carried unanimously.

December 6, 1880.

PROFESSOR NEWTON, PRESIDENT, IN THE CHAIR.

The following communications were made to the Society:

(1) *On the various notations adopted for expressing the common propositions of Logic.* By JOHN VENN, M.A., Fellow of Gonville and Caius College.

Most logicians must be well aware of the general fact of the perplexing variety of symbolic forms which have been proposed from time to time by various writers, but probably few persons have any adequate conception of the extent to which this license of invention has been carried. I have therefore thought it well to put together into one list the principal forms, so far as I have observed them, in which one and the same proposition has thus been expressed. For this purpose the Universal Negative has been selected, as being about the simplest and least ambiguous of all forms of statement. This arrangement has not been drawn up with a mere wish to make a collection. Almost every one of these forms, it must be remembered, has been made the instrument of a more or less systematic exposition of the subject. In so far therefore, as the notation is not entirely arbitrary—which it is in very few instances—we shall find it instructive to compare the different aspects of the same operation to which they respectively direct attention. For convenience of reference and comparison

ey are expressed in the same letters in each case, S and P anding respectively for the subject and predicate of the original oposition; viz. no S is P.

The analysis by which I should reach these various forms ould be somewhat of the following kind:

I. In the first place we may regard the proposition as an *istential* one. In this case what it does is to deny the existence a certain class, viz. of the things which are both S and P.

II. We may cast the proposition into the form of an *identity*. hat we then do is to make the terms of the proposition respectively and not-P, and to identify the former with an undetermined rt of the latter. The appropriate copula is then of course (=).

III. Another plan is to regard the proposition as expressing *consequence* or *implication*: 'If S then not-P,' or, 'the presence S implies the absence of P.' This relation, of course, is not nvertible, for it does not follow that the absence of P implies e presence of S. Accordingly, some kind of unsymmetrical mbol becomes appropriate to represent the copula in this case.

IV. Again, keeping more closely to the common expression the proposition, we may regard it as expressing a relation not, above, between S and not-P, but between S and P. This lation is convertible, and we should therefore naturally seek in is case for some symmetrical symbol.

V. Again, we may couch the proposition in conceptualist or tional phraseology, regarding P and S not as classes of things it as attributes or groups of attributes.

VI. Lastly, we may meet with nondescript attempts which m at little more than just translating the proposition as it stands, adopting some arbitrary notation to stand for it.

Grouping them thus, we may arrange our species as follows:—

I. Existential	1 $SP=0$	Boole.
	2 $S(0=P)$	Macfarlane.
II. Identity	3. $S=v(1-P)$	Boole.
	4. $S=\frac{0}{0}(1-P)$	Boole.
	5. $S=v(X-P)$	Wundt.
	6. $S=Sp$	Jevons.
	7. $S=1-P-y$.	Delboeuf, Murphy.
	8. $S=\frac{P}{\infty}$	Holland.
	9. $S<X-P$	Drobisch.
	10. $S<-P$	Segner.

III. Implication (non-convertible)	11.	$+S-P$	Darjes.
	12.	$S \angle \overline{P}$	Grassmann.
	13.	$S : P'$	Maccoll.
	14.	$S \multimap\!\!- \overline{P}$	Peirce.
	15.		Frege.
IV. Common Form (convertible)	16.	$S \,.\, P$	De Morgan.
	17.	$S)(P$	Wundt.
	18.	$S > P$	Ploucquet.
	19.	$tS \parallel tP$	Bentham.
	20.	S :  : P	Hamilton.
V. Notional	21.	$L - S \infty P$	Leibnitz.
	22.	$\frac{S}{m} = \frac{P}{n}$	Lambert.
	23.	$S > \frac{P}{n}$	Lambert.
VI. Nondescript, and Arbitrary	24.	$Sx = -P$	Maimon.
	25.	$nS - P$	Ploucquet.
	26.	$mS - P$	Chase.

As most of these forms are marked off from each other b more or less distinct specific differences, it will be well to go a littl into detail in describing them.

1. This would need but little explanation to any logical reade It simply indicates the destruction of the class SP, or the emptines of the corresponding compartment. I prefer it myself as a primar statement of such a proposition.

2. The characteristic of Mr Macfarlane's notation here lies i the attempt to mark the limits of the 'universe.' He consider that the subject of the proposition marks the limits of the implie universe. "Every general proposition refers to a definite universe which is the subject of the judgment, and, it may be, of a series c judgments. For example, 'all men are mortal' refers to th universe 'men.' 'No men are perfect' refers to the same uni verse" (*Algebra of Logic*, p. 29). Hence his symbolic form is rea off, "Within the universe of S there is no such thing as a P."

3—6. These four forms are to all intents and purposes identica The only distinction between them is that (3) introduces an a bitrary sign (v) to express the indeterminateness of the selection t be made from not-P; that (4) employs a well-known mathematica symbol to express the same idea; whilst (6) disguises this inde

terminateness by describing S as the S-part of what is not P instead of directly reminding us that it is an unknown part. It also abbreviates by substituting a single letter p for the fuller equivalent $1-P$. No. (5) is employed by Wundt (*Logik*) in his account of symbolic procedure. It only differs from the third and fourth by making use of X as the universe-symbol, instead of *unity*. As regards the representation of the class not-P by p, in (6), there is one serious defect; for the purposes of symbolic logic, a fatal defect. We cannot thus represent the negation of a composite class. The other schemes meet the difficulty. "What is not both B and C" can be readily represented by $1-BC$, and "what is neither B nor C" by $(1-B)(1-C)$; and the similar devices of a bar over two or more letters, or an accent put outside a bracket which contains them (as in specimens 12 and 13) will subserve the same purpose. But on the plan of employing small letters to mark negations, *ab* would stand for "What is neither A nor B," and there seems no ready mode of simply expressing the negation of AB *as a whole*. We have to break it up into detail and write it $a + Ab$, or in some equivalent form. This is a serious symbolic blemish.

7. This form is employed by Prof. Delboeuf (*Logique Algorithmique*, 1877), and seems to me identical with one proposed by Mr J. J. Murphy (*Relation of Logic to Language*, 1875; *Mind*, v. 52). It only differs from the preceding ones by adopting the subtractive instead of the multiplicative symbol. Whereas those four say, Make an indeterminate *selection* from not-P and we obtain S; this says, Make an indeterminate *rejection* by omission, and we obtain S. It is not incorrect, but seems to me to suffer from the drawback of demanding a tacit condition, viz. that y shall be included in $1-P$. When this condition is expressed, it coincides with Boole's form (If B is included in A, so that $B = vA$, then $1-B=(1-v)A$: but, $1-v$ having the same limits of uncertainty as v, this may be written vA), and becomes exactly equivalent to $S=v(1-P)$. It should be noticed that Delboeuf divides this general form into several distinct cases according to the extent of the whole universe covered by S and not-P, and so forth (*Log. Alg.* p. 58).

8. This was a scheme proposed by Holland, a friend and correspondent of J. H. Lambert. Though not sound in this particular application it deserves notice, both for its ingenuity, and historically as an anticipation of some more modern results. (It is given in Lambert's *Deutscher gelehrter Briefwechsel*, I. 17.) The general propositional form which he proposed was $\frac{S}{p}=\frac{P}{\pi}$, which is really

nothing else than Boole's $vS = v'P$, with the difference that the arbitrary factor is put into the denominator instead of the numerator (i.e. the factor *multiplies* by $\frac{1}{p}$ and $\frac{1}{\pi}$, it does not *divide* by p and π; there is no notion here of an inverse logical operation, though Holland had realized it elsewhere). The consequent condition as to the range of value of p and π is of course that they must lie between 1 and ∞, just as in Boole's form v and v' lie between 1 and 0. What we express symbolically therefore is 'some S is some P,' where 'some' may range from *none* to *all*. So far good. Where he goes astray is by interpreting the limiting case $S = 0P$ (viz. when $p = 1$, $\pi = \infty$) as meaning "all S is no P," instead of "all S is *nothing*." The fact is that his form is extensible enough to cover particular and universal affirmatives, with distributed and undistributed premises; but in order to make it stretch so as to cover negative propositions we must either use a negative predicate, not-P, or else join S and P together into one complex subject and equate this to 0 as in (1).

All of these six concur in employing the equation symbol (=), and rightly so, for what they represent is the identity of the subject S with a portion of not-P. The two following must really be considered to belong to the same general class, though actually employing a different, and decidedly less suitable symbol, viz. (<).

9. This was employed by Drobisch in the first edition of his *Neue Darstellung der Logik* (1836), but is omitted in later editions. Two points deserve notice about it. First as regards the connecting symbol itself. We are familiar with it in mathematics as meaning 'less than;' and it is here transferred to the signification 'is included in,' or 'is identical with a part of.' It is therefore exactly equivalent to the equation symbol when, as in the last examples, this is affixed to a predicate affected by some indeterminate factor. This transfer of the sign < does not seem to me a very convenient or accurate one, though its signification is quite clear; it need hardly be remarked that it here refers to the *extent* not the *intent* of the terms S and P. Secondly, as regards the predicate, the notation is curious as shewing the great difficulty which logicians brought up in the old traditions had in realizing the conception of a 'universe,' which could be represented by a single symbol. The letter X does not here stand for really "all," for this would be to introduce an "unendlicher Begriff," or "infinite term," quite alien to old tradition. Drobisch only ventures symbolically to embrace a finite but uncertain portion of this infinite universe. Let us then take a class term X of uncertain extent, only demanding that its

xtent shall be greater than those of S and P together, and we nay regard this as finite, and therefore suitable to logical treatment. When our negative predicate, not-P, is thus brought down to finite xtent in the form of $X-P$, we can venture to refer S to it. We mply, in fact, that S is a portion of not-P; and we write it not by n equation formula, but by an inclusion formula, as $S < X - P$.

10. This is of considerable interest historically, since Segner's *Specimen Logicæ* (1740), is the first systematic attempt, so far as I ave seen, to construct a symbolic Logic. (He had nothing before im of this kind to appeal to beyond a few ingenious suggestions y Leibnitz.) The sign $<$ is used in the same sense as by Drobisch, $A < B$ marking that the extent of B is inclusive of that f A. But in one respect he seems to me distinctly in advance f Drobisch, and very much in advance of his time. This is in his ree use of negative terms in their full extent (he preserves the ld name of 'infinite' for them), for the representation of which e uses the negative sign. Thus if A stands for man, $-A$ stands or not-man. It may be added that he had fully realized the fact hat it is symbolically indifferent whether we apply A to a positive r to its contradictory, provided we preserve the antithesis between + and $-$. Thus if A stands for *non-triangulum*, $-A$ will stand for *riangulum*, and so forth. Hence his expression $S < -P$ marks uite correctly that S is extensively a portion of not-P.

This notation of course is very crude, being not of much value ven within the limits of the syllogism. The various syllogistic noods are however worked through with its aid, but with certain lepartures from the common view which need not here be decribed. It may interest the historical student of this part of the ubject to say that Segner not only describes the symbolic proedure by which from two such premises as $A < B$, $C < D$, we can nfer $AC < BD$; but also expressly calls attention to the "law of luality" as it is sometimes termed, viz. that $AA = A$; that is, he oints out that when A and C are the same no change is proluced :—"subjecti enim idea, cum se ipsa composita, novam ideam roducere nequit." There are many other interesting points about he work which must be passed over here.

We now come to a group of forms which I have thrown toether as adopting somewhat of an *implication* arrangement: some f them indeed expressly describe themselves as indicating an mplication. Thus,

11. This is best put into words as, "posit S, and we sublate P." t turns entirely upon the representation of contradictories by + nd $-$, a representation which, as in the closely analogous case ust discussed, will do fairly well up to a certain point. If we

only want to deal with pairs of contradictories, whether terms or propositions, and only want, as above remarked, to posit and sublate them, the signs + and − are convenient. But then we lose the use of these signs for far more appropriate purposes, viz. for logical aggregation and exception. Moreover the antithesis thus suggested of a contradictory, rather than a supplementary relation between S and not-S, soon leads to difficulties. How are we to represent not-S . not-P? By $(-S-P)$ or by $(-S)\times(-P)$? There is no convenient opening here for compounding terms or premises.

The only brief and convenient rule for working this notation seems to apply to the process of *conversion*. From 'Posit S and sublate P,' we deduce of course 'Posit P and sublate S.' Generalizing this to cover the four possible cases we see that it may be summed up in the words 'change the order of the terms and both the signs:' i.e. from $(+S+P)$ we infer $(-P-S)$, and so on.

Though therefore the scheme of employing + and − in logic, for this purpose, repeatedly presents itself, it does not seem to me to merit any more detailed investigation.

12—14. These are, to my thinking, precisely equivalent to one another. It is true that Mr Maccoll insists upon it that his interpretation of his class symbols as standing for *statements*, marks a 'cardinal point' of distinction; but I regard this as an arbitrary restriction of the full generality of our symbolic language. Phrase it how we will,—the presence of S implies that of P, the existence of S implies that of P, the truth of S implies that of P; —the antithesis at bottom is always the same, or rather it comes under one generalized signification.

It may be added, in explanation of the differences of detail, that the line over a letter, and the accent, respectively mark contradiction; and the three copula-symbols may in each case alike be read 'implies.' We may read them therefore, 'S marks, or implies, the absence of P.' The converse of course does not hold; that is, not-P does not imply S. If this additional information were given to us we should in each case employ the copula (=), and write them $S=P'$, $S=\bar{P}$. The sign of equality marks in fact the double implication, just as 'All S is all P' contains the two propositions, 'All S is P,' and 'All P is S.'

Of course, in saying this, it must not for a moment be supposed that the various systems which make use of these notations are themselves coincident. On the contrary, there are various differences, both in the detailed treatment and in the rest of the notation, even between (12) and (13); whilst Mr Peirce has made his symbols a means of attacking various problems (such as the Logic of Relatives) which have not seemed to me to lie across the path we have had to take.

(For Grassmann's scheme, see his *Begriffslehre*, 1872; for that of Mr Maccoll, the *Proc. Lond. Math. Soc.*, 1877; and for that of Prof. Peirce, the *American Journal of Math.*, Vol. III.)

15. Frege's scheme (*Begriffsschrift*, 1879) deserves almost as much to be called diagrammatic as symbolic. It is one of those instances of an ingenious man working out a scheme,—in this case a very cumbrous one,—in entire ignorance that anything of the kind had ever been achieved before. A word or two only of explanation can be devoted to it here. A horizontal dash with a short vertical stroke at the end signifies a proposition. The line S running into P means that P is dependent upon S;—this is in fact his sign of dependence or implication. The little stroke under the P-line marks negation. So that the whole arrangement stands for 'If S then not P.' We can proceed in this way to build up more complicated dependencies. For instance, by joining this whole arrangement on to another such line, we can represent the compound dependency 'The fact that S implies the absence of P, implies P,' and so on. One obvious defect in this scheme is the inordinate amount of space demanded by it; nearly half a page is sometimes demanded for an implication which any reasonable scheme would compress into half a line.

The members of the group now before us, expressing a relation between S and not-P, are essentially non-convertible, and therefore appropriately employ non-symmetrical symbols for the copula*. In this respect they depart somewhat from tradition and each is the scheme of a logical innovator. The next group keeps closer to old custom, in respect that its members express directly a relation between S and P, and therefore call for a symmetrical copula-symbol.

16—18. These three of course employ purely arbitrary symbols, and are meant to do so, the symbols being mere substitutes for the copula of ordinary Logic. Wundt's symbol is one of a group (*Logik*, p. 244) some of which mark reciprocal relations between the terms, and some non-reciprocal, and its symmetrical form is meant to shew that it belongs to the former class. Thus $S = P$ marks identity; $S > P$ superordination of S to P; $S < P$ subordination of S to P, and $S \between P$ the intersection of S and P. These, with $S)(P$, make up the five possible distinct forms of class relations. To these however Wundt adds some others which are not so much class relations as dependencies or implications. De Morgan, I suspect, had not this distinction between

* No. (13) is of course non-symmetrical in its recognized signification in mathematics, though symmetrical in actual form.

symmetrical and unsymmetrical forms clearly in view. His notation here is that which he adopted in his *Formal Logic;* he changed it subsequently in his papers in the *Camb. Phil. Transactions.*

As regards Ploucquet's expression, this is mainly employed in the more symbolical parts of his logical treatises (e.g. his *Methodus Calculandi*). He there uses only two signs; one, an arbitrary and somewhat misleading sign for negation, (>); and one for affirmation (juxtaposition of the letters). The predicate is always distributed, the whole and part of it being indicated by large and small letters respectively. Thus 'All A is B' stands, Ab, viz. 'All A is some B.' 'No A is C' stands $A > C$, viz. 'No A is any C.' The processes of reasoning are then resolved into substitution of identities and recognition of non-identities. It may be remarked that had Ploucquet broken sufficiently with the past to make a free use of negative, or infinite predicates, he might have adopted another form for these negative propositions. It is true that he does occasionally employ such predicates, but not sufficiently often to have devoted a special symbol to them. Had he written, for instance, $\bar{P}$ for 'all not-P,' and $\bar{p}$ for 'some not-P', his expression for 'No S is P' might have been $S\bar{p}$, viz. 'All S is some not-P,' in better accordance with the familiar symbolic view at the present time, and as illustrated in group (II).

19, 20. These two must be regarded as precisely equivalent, with one exception to be presently noticed. They are both founded upon the doctrine of the Quantification of the Predicate, and are meant to call attention to that characteristic. They may be translated as saying 'the whole of S is distinct from the whole of P.' Mr Bentham's t means totality, just like Hamilton's (:), and the parallel lines of the one bear the same signification as the wedge of the other, viz. 'distinct from,' or as Hamilton sometimes puts it, 'not congruent with.' The differential characteristic of Hamilton's symbol lies in the distinction between the thick and thin ends of the wedge, which is meant to mark whether the proposition is read in extension or in intension. This attempt to compress both these interpretations into one form is now, I presume, generally regarded as a mistake. (See Hamilton's *Logic,* II. p. 473; Bentham's *Logic,* p. 134.)

We now turn to a group the interpretation of which is necessarily one of intension, that is, in which the letters stand for notions or attributes and not for classes.

21. Leibnitz's formula is given in his *Specimen demonstrandi* (Erdmann, p. 96). It is not definitely assigned as a symbolic expression of the proposition; and like some other of the logical speculations in his shorter works seems indeed to have been

hrown out as little more than a hint. His view is this. The ign (−) is the sign of 'detraction,' i.e. abstraction, or the with- rawal of an attribute from a notion; and (∞) is the sign of dentity. Now let L and P be two notions which have something n common, but that when S is thrown out of the former the re- nainder is P. This is expressed by $L - S \infty P$, and implies that S nd P are distinct notions, or that No S is P. His own account f the matter (I have changed some of the letters) is this. "Sit $L - S \infty P$. Dico S et P nihil habere commune. Nam ex defini- ione detracti et residui omnia quæ sunt in L manent in P præter a quæ sunt in S, quorum nihil manet in P": so that no S is P.

This particular suggestion is very brief, and seems to me ecidedly obscure, but it deserves mention, both historically and s having possibly given occasion to the similar but much more omplete suggestions of Lambert. I proceed to their discussion.

22. This scheme of Lambert's might at first sight be con- idered identical with that of Holland (No. 8), or rather with he general propositional form of which that is a particular case; or the two expressions are formally the same. In reality, however, hey are in striking contrast with each other. With Holland, he letters p and π, in the denominators, really stood for numerical actors. What he meant to signify was that 'some portion of he extent (estimated by $1 \div p$) of S, is identical with some portion similarly estimated by $1 \div \pi$) of that of P'; though he blundered vhen he came to interpret this into a negative proposition. But vith Lambert, m and n have a better right to stand in the deno- ninator. They mark *attributes*, and division by them stands or *abstraction*, so that the proposition is interpreted here not in espect of extent but of intent. His idea is this. Though S nd P are distinct as classes they must have some attributes in ommon; that is, they must both belong to some higher genus. Abstract then certain attributes from each, as indicated in the livision respectively by m and n, and the remaining groups of ttributes will coincide.

This is quite true, and highly ingenious, but what one does not ee is how this symbolic expression becomes a fitting represen- ative of the universal negative proposition rather than of any ther. Whatever the relations of extent of two notions, S and P, t will always hold good that some of the attributes in one are lifferent from some of those in the other. This points, I think, o an essential defect in the attempt to interpret propositions n respect of the intent of both their subjects and predicates; t gives us, for instance, no means of distinguishing between 'some X is Y,' and 'some X is not Y,' or indeed for adequately charac- erising any 'particular' proposition whatever.

It is rather curious that Segner, whose work Lambert had read (*Briefwechsel*, I.), could have set him right here. He has expressly discussed almost exactly the same question, and realized its logical bearings clearly, though he did not reach the very important symbolic step of introducing the inverse or division sign to mark it. He stated this theorem: Given that two classes indicated by composite terms, AB and CD, have something in common, and we abstract an attribute from each, say A and C, then the resultant classes, B and D, must also have something in common. But such community may be of any one of four kinds which he marks respectively by $B = D$, $B < D$, $B > D$, $B \times D$; that is, coextension, inclusion of B in D, inclusion of D in B, and intersection.

23. The above expression will be found described in Lambert's *Deutscher gelehrter Briefwechsel*, I. 37; and in the *Nova Act. Erudit.* 1765. The present one is a slight modification of it given in his *Log. Abhandlungen*, I. 98. The general idea is exactly the same. Abstract sufficient attributes from P until only those are left which are common to it and to S. This does not yield an *identity* as before, for S is now more determinate than P, but it makes the remaining attributes of P *included in* those of S. Interpreted therefore in intension, we have 'All $\frac{P}{m}$ is S,' and this we express, by use of the sign >, in the form $S > \frac{P}{m}$. Another equivalent form given by Lambert, and which the reader will readily interpret for himself, is $\frac{S}{m} < P$. It is obvious that in order to get an identity of subject and predicate, instead of a mere inclusion of one by the other, we must abstract from *both* of them, as in (22).

The three remaining forms are of little speculative interest; indeed No. (25) is only inserted as a curiosity of symbolic mismanagement.

24. This is a form employed by S. Maimon (*Versuch einer neuen Logik*, 1794). The negative sign here indicates, as in several other systems, the contradictory of a class, so that $(-P)$ means not-P. The term x is intended to represent an arbitrary logical factor or determination. Hence the interpretation is, "S, howsoever determined, is not-P"; i.e. by no kind of qualification can we reduce it to any part of P. Of course the qualification here can only be in the way of logical *determination*: not *abstraction*, as in the two preceding. There are a variety of serious defects in this notation, and it represents altogether a great

lling off from some of its predecessors, though Maimon has ntrived with more or less success to carry it through all the llogistic moods. One obvious inaccuracy is to be seen in the e of the sign (=). We have no right to adopt the equational rm unless the subject and predicate are identified, which they e not in this case, the former being a part only of the latter.

25. Ploucquet frequently uses this form in some of his logical ritings (e.g. *Fundamenta Philosophiæ speculativæ*, 1764). It ust be observed that the sign (−) here stands for affirmation, rather for that and negation indifferently, the negation being it into the subject, where N stands for *nullum*. It is therefore erely a rendering of the common form 'No S is P,' whereas $-P$ would have stood for '*S is P*.'

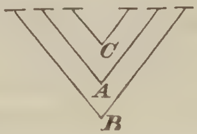
26. A mere vagary on the part of Mr Chase (*First Logic ook*, 1875) which should stand as a caution to non-mathematical mbolic innovators. The sign (−) here is defined as standing r *negation*, whilst + does duty for affirmation. So, to make quite re that we are really denying, he puts in the word *no* as well, id writes it No $S-P$; that is, No S is not P.

On the employment of geometrical diagrams for the sensible re- resentation of logical propositions. By JOHN VENN, M.A.,

A few preliminary words will be desirable in order to point it the limits of this notice. It is intended here to take account those schemes only which deal directly with propositions, and hich *analyse* them; that is, which in some way or other hibit the relation of the subject to the predicate. Hence two inds of diagram of almost immemorial antiquity in Logic, will ive to be entirely rejected. The first of these is the so-called Por- hyrian tree. This only represents the mutual relation of classes one another in the way of genus and species, by continued ibdivision; and though of course giving rise to propositions cannot be said in any way to portray them. The other is ie *triangle* of which the three extremities are used to represent ie three terms of the syllogism. The same outline of a diagram ere serves for any kind of proposition, and all that is meant be illustrated by the figure, is, that we may by means of asoning connect the extremes A and C (so to say, along one de) instead of connecting A with B, and then B with C (along ie two sides). In this sort of diagrams no kind of analysis of opositions is attempted, and it can hardly be claimed for them iat they are any real aid to the mind in complicated trains

of reasoning. A historic sketch of their origin will be found in Hamilton's *Discussions*, Ed. III. p. 666.

As regards then the employment of what I term analytical diagrams, viz. those meant to distinguish between subject and predicate, and also between the different kinds of proposition,—there can be little doubt that their practical employment dates from Euler. That is to say, he first familiarized logicians to their use, and the particular kind of circular diagram which he employed has consequently very commonly been named after him. But their actual origin is very much earlier than this. The earliest instance that I have seen is in the *De Censura Veri* of Ludovicus Vives*, where the mutual relations of the three terms in *Barbara*, as given by the two premises, are represented very much as on the Eulerian plan. He speaks of representing them by means of containing *triangles*, but the actual figures drawn are those of the letter V, as thus,



This is the only diagram to be found, I believe, in the work.

Priority in this direction has also been claimed by Hamilton for Alsted, who, as he maintains, had in his *Systema logicum* anticipated the linear kind of diagram proposed by Lambert and which will presently be explained. I cannot however perceive that Alsted had the slightest idea of representing what Euler and the others aimed at representing. All that he says (speaking of the first figure), is that the middle term is 'below' the major term and 'above' the minor, and he just draws three lines of equal length, one under the other, to illustrate what he means. "Etenim omne medium, quod est inter duo extrema secundum altitudinem, id est, inter extremum superius et inferius, illud inquam medium debet habere aliquod extremum supra se, et aliquod infra se. Atqui medius terminus in prima figura est talis: habet enim terminum supra se, nempe praedicatum conclusionis in majore propositione positum, et habet terminum infra

* His words are: "Si aliqua pars *a* capit totum *b*, et aliqua pars *b* capit totum *c*, *c* totum capietur ab *a*: ut, si tres trianguli pingantur, quorum unus *B* sit maximus, et capiet alterum *A*, tertius sit minimus intra *A*, qui sit *C*, ita dicimus, si omne *b* est *a*, et omne *c* est *b*, omne *c* est *a*: adhibeatur regula quam diximus esse canonem artium et vitæ totius." *De Censurâ Veri*, Lib. II. (My attention was directed to this by F. A. Lange's *Logische Studien*, p. 10.) I do not understand how the capital and small letters here agree with each other.

positum, nempe subjectum conclusionis in minore propositione situm....Diagramma est tale :—

Animal
a ———————— *b*
sentit
c ———————— *d*
homo
e ———————— *f*."

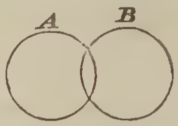
ere is nothing in this,—the only diagram of the sort which gives,—even to suggest the distinction between affirmative d negative, universal and particular, which is the least we h look for in these sensible illustrations.

The first logician apparently to make free use of diagrams s Chr. Weise, rector of Zittau, who died in 1708. He seems have published some works on logic himself, but his system is d to be given in the *Nucleus Logicae Weisianae* of J. C. Lange, 712). I have not succeeded in seeing this work, but judging by at Lambert says of it (*Architectonik*, I. 128), I gather that makes free use of circles and squares for the purpose of repre-nting propositions. Hamilton (*Logic*, I. 256) confirms this tement.

In the only work of Lange's to which I have been able to ob-n access, viz. his *Inventum novum Quadratilogici*, there is nothing ich strikes me as of any great merit. There are a number of ometrical figures represented, both plane and solid, but the thor does not seem to have grasped the essential conception of ustrating in this way the *mutual intersection*, or otherwise, of o or more classes by means of his figures. All that he repre-nts is *continued sub-division:* e.g. that of A into B and C, of B to D and E, and C into F and G, and so forth. This he sets th by a parallelogram for A; under it is put a similar one vided into two equal parts to denote B and C, the next in order ving four divisions and so forth. All that this properly repre-nts is the doctrine of Division or continued Dichotomy; i.e. the tire exclusion of B by C, and the entire inclusion of D and E B, and so forth. There is no attempt to represent the various ations of two terms, B and C, to one another, as set forth in e various forms of proposition which have B and C for their bject and predicate.

We now come to Euler's well-known circles which were first scribed in his *Lettres à une Princesse d'Allemagne* (Letters 102 105). The weak point about these consists in the fact that ey only illustrate in strictness the actual relations of classes to e another. Accordingly they will not fit in with the propositions common logic, but demand the constitution of a new group of propriate elementary propositions. This defect must have been ticed from the first in the case of the particular affirmative and

negative, for the same diagram is commonly employed to stand for them both, which it does indifferently well:



Some A is B,
Some A is not B,

for the real relation thus exhibited by the figure is of course "some (only) A is some (only) B", and this quantified proposition has no place in the ordinary scheme.

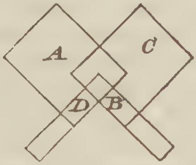
Euler himself indicated the distinction (so I judge by his diagram) by the position in which he put the letter A; if this stood in the 'A not-B' compartment it meant 'some A is not B', if in the AB compartment it meant 'some A is B'. But the common way of meeting the difficulty where it is at all recognized, is by the use of dotted lines to indicate our uncertainty as to where the boundary should lie. So far as I have been able to find, this plan (as applied to closed figures) was first employed by Dr Thomson in his *Laws of Thought*, but was doubtless suggested by the device of Lambert, to be presently explained. It has been praised for its ingenuity and success by De Morgan, and adopted by Prof. Jevons amongst others. Ueberweg has employed a somewhat more complicated scheme of a similar kind.

Any modifications of this sort seem to me (as I have elsewhere explained) wholly mis-aimed and ineffectual. If we want to represent our uncertainty about the correct employment of a diagram, the only consistent way is to draw *all* the figures which are covered by the assigned propositions and say frankly that we do not know which is the appropriate one. Of course this plan would be troublesome when several propositions have to be combined, as the consequent number of diagrams would be considerable. Thus in *Bocardo*, two diagrams would be needed for the major premise and two for the minor, making four in all.

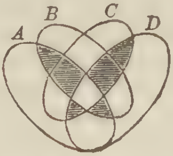
The traditional logic has been so entirely confined to the simultaneous treatment of three terms only (this being the number demanded for the syllogism) that hardly any attempts have been made to represent diagrammatically the combinations of four terms and upwards. The only serious attempt that I have seen in this way is by Bolzano. He was evidently trying under the right conception, viz. to construct diagrams which should illustrate *all* the combinations producible by the class terms employed, but he adopted an impracticable method in using modified Eulerian diagrams. The consequence is that he has effected no general solution, though exhibiting a number of more or less ingenious

ıres to illustrate special cases. Thus a collection of small circles luded in a large one represents a number of species (mutually lusive) comprehended under one genus, though, since the small les cannot fill up all the contents of the large one we cannot s conveniently represent the exhaustion of the genus by the regate of the species; a row of such circles, each of them erlinked with the next, represents the case of a succession of cies each of which has something in common with the next, l so forth.

The most ingenious of his figures for four terms is the follow-. I give it here in order to show the necessary shortcomings this method:—



is offered in representation of the proposition "A which is B he same as C which is D". It must have taken some trouble arrange it, so that as regards economy of time any such resort ıld be decidedly the reverse of an aid. Moreover, as the reader l readily perceive, it is not quite correct; one possible sub-ision, viz. $\bar{A}B\bar{C}D$, having been omitted. There is nothing in statement to forbid the occurrence of BD which is neither A C, so that the correct state of things would be better exhibited ıs, on the plan described in my Article in the *Philosophical ıgazine* (July, 1880).



With the exception of that of Bolzano, I have seen no attempt extend diagrammatic notation to the results of four terms, and is only quite recently that really adequate figures have been posed for those of three terms:—for instance both Drobisch d Schröder have used what I have called, in the article in ques-n, the three-circle diagram*. In saying this, I do not of course an to imply that the problem was one of any particular diffi-

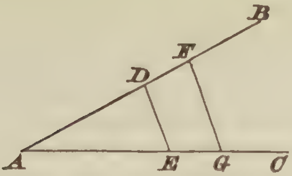
* These writers merely represent in this way the class combinations or divisions as such: they do not adopt the subsequent step of using them as asis for representing propositions.

culty, but merely state the fact that general satisfaction being felt with the Eulerian plan no serious attempts were made to modify it. Indeed, except on the part of those who wrote and thought under the influence of Boole, directly or indirectly, it was scarcely likely that need should be felt for any more generalized scheme.

The essential characteristic of the Eulerian plan being that of representing directly and immediately the inclusion and exclusion of classes, it is clear that the employment of circles as distinguished from any other closed figures is a mere accident. Nor have circles in fact always been employed. Thus Ploucquet,—whose system however, as he himself pointed out, in contrast with that of Lambert, is essentially symbolic and not diagrammatic,—has made use of squares. Kant (*Logik*, I. § 21) and De Morgan (*Formal Logic*, p. 9) have introduced or suggested both a square and a circle into the same diagram, one standing for subject and the other for predicate; with the view of distinguishing between these. Mr R. G. Latham (*Logic*, p. 88) and Mr Leechman (*Logic*, p. 66) have a square, circle, and triangle all in one figure for the same purpose, presumably, of distinguishing between the three terms in the syllogism. Bolzano again, in one of the examples above adduced, has had resort to parallelograms: to which indeed, or to ellipses or to some such figure, it is evident he must have appealed if he wished to set before us the outcome of four class terms. But to regard these as constituting distinct schemes of notation would be merely idle. They all do exactly the same thing, viz. they aim at so arranging two (or more) closed figures that these shall represent the mutual relation of inclusion and exclusion of the various classes denoted by the terms we employ.

There is one modification of this plan which deserves passing notice both on its own account and because it has been so misjudged by Hamilton. It is that of Maass. In order to understand it we must recall one essential defect of the customary plan. Representing as this does the final outcome of the class relation, it is clear that every fresh proposition demands a diagram new from the beginning. If we have drawn a scheme for "All A is B" we must abandon it and draw another for "All B is A". Seeing this, apparently, Maass took two fixed lines enclosing an angle, and regarded the third line which combined with them to form the necessary closed figure, as movable. Hence only one line had to be altered in order to meet the new information contained in such a second proposition. (*Logik*, p. 294.)

Thus let AB and AC be the fixed lines; and the triangle ADE represent the class X, and AFG represent the class Y. If FG remain where it is we have "All X is Y", whilst in order to represent "All X is all Y" we have only to conceive it transferred



as to coincide with DE. This seems to me to constitute the sential characteristic of his scheme, which is worked out in a riety of figures of a more or less complicated kind. It is deledly cumbrous, and not entirely effective as regards this its main n, but it deserves recognition as an attempt to remedy a real fect in the ordinary scheme.

Hamilton who, as we know, never could succeed in grasping the ture of a triangle, entirely misconceived all this; and seeing at Maass began by talking of an angle he concluded that angles re being employed as the representative of class relations. ence his judgment, hurled in a blast of wrathful and contempous epithets, that this is "the only attempt made to illustrate gic, not by the relations of geometrical quantities, but by the lations of geometrical relations,—angles" (*Logic*, II. 463).

The above schemes aim at representing the relative extent of ss terms by the really analogous case of the relative extent of osed figures, which therefore tell their tale somewhat directly. departure from this plan was made, shortly after the date of ler's letters, by Lambert, who introduced a more indirect scheme diagrammatic notation. He indicates the extent of a class rm by a straight line; the inclusion of one term in another being presented by drawing a shorter line under the other, the exsion of two by one another by drawing them side by side, whilst e corresponding case to the intersecting circles is presented by awing one line partially under the other, as follows ——— hus Celarent might be represented:

B ————	———— A	No B is A,
C ———		All C is B,
		$\therefore$ No C is A.

So far the scheme is of essentially the same kind as that of ler*, the only important difference being that the common part

* The analogy which Lambert actually had in view seems however to have en different. He evidently was influenced, like Alsted, by the technical expresn, "thinking objects as *under* such and such a concept," which to modern ears uld sound as little more than a play on words. Thus he draws a line to reprent the general concept and puts a row of dots underneath to represent the lividuals which stand under that concept. And again, "Ferner fordert der sdruck, dass alle A unter B gehören, von Wort zu Wort genommen, dass man Linie A unter B setzen müsse." (*Dian.* § 181.)

of the extent of the two terms is not here indicative of *identity;* for the line *C* is thus only drawn under *B* and not made coincident with it. This was noticed at once by Ploucquet, whose theory of propositions turned entirely on the identity of subject and predicate and consequent quantification of the predicate. He maintains that Lambert would do better to draw the second line, in an affirmative proposition, wholly or partially *coincident* with the first, and so secure this identity (*Sammlung*, p. 182). Since however the help to the eye would then be nearly lost, such an alteration would simply result in a poor and faulty imitation of the Eulerian scheme.

Lambert however did not stop here. Like most other clear thinkers he recognized the flaw in all these methods, viz. that we cannot represent the uncertain distribution of the predicate, but employ one and the same diagram for "All *A* is *B*", whether the predicate be 'all' or only 'some' *B*. He endeavoured to remedy this defect by the employment of dotted lines, thus:

B............ ———— ....

A ————

This means that *A* certainly covers a part of *B*, viz. the continuous part; and may cover the rest, viz. the dotted part, the dots representing our uncertainty. In this case the scheme answers fairly well, such use of dots not being open to the objection maintainable against it when circles are employed. But when he comes to extend this to particular propositions his use of dotted lines ceases to be consistent or even, to me, intelligible. One would have expected him to write 'some *A* is *B*' thus,

B ————.........

A ————.........

for by different filling in of the lines we could cover the case of there being '*B* which was not *A*,' and so forth. But he does draw it

B ————

A ....

which might consistently be interpreted to cover the case of 'no *A* is *B*,' as well as suggesting the possibility of there being no *A* at all.

Lambert's use however of this modification of his scheme is so obscure, and, when he comes to work out the syllogistic figures in detail, is so partially adhered to, that it does not seem worth

e expenditure of further time and thought. As a whole, it ems to me distinctly inferior to the scheme of Euler.

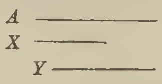
Hamilton's own system of notation is pretty well known. It is ven in his *Logic* (end of Vol. II.) with a table, and is described his *Discussions*. Some account of it will also be found in Thomson's *Laws of Thought*. It has been described (by himself) "easy, simple, compendious, all-sufficient, consistent, manifest, ecise, complete;" the corresponding antithetic adjectives being ely expended in the description of the schemes of those who had ne before him. To my thinking it does not deserve to rank a diagrammatic scheme at all, though he does class it with e others as "geometric": but is purely symbolical. What s aimed at in the methods above described was something that uld explain itself at once, as in the circles of Euler, or need but hint of explanation, as in the lines of Lambert. But there clearly nothing in the two ends of a wedge to suggest subjects d predicates, or in a colon and comma to suggest distribution d non-distribution.

So far we have considered merely the case of categorical prositions; it still remains to say a few words as to the attempts ade thus to represent other kinds of proposition. The Hypoetical may be dismissed at once, probably no logician having pposed that these could be exhibited in diagrams so as to come t in any way distinct from categoricals. Of course when we nsider the hypothetical form as an optional rendering which ly differs verbally from the categorical, we may regard our agrams as representing either form indifferently. But this urse, which I regard as the sound one, belongs essentially to e modern or class view of the import of propositions. Those o adopt the judgment interpretation can hardly in consistency me to any other conclusion than that hypotheticals are distinct om categoricals, and do not as such admit of diagrammatic presentation.

The Disjunctive stands on a rather different footing, and some tention has been directed to its representation from the very st. Lambert, for instance, has represented what we must regard a particular case of disjunction, in the subordination of a pluity of species to a genus, after this fashion:—

is of course indicates the fact that the three classes X, Y, Z, gether make up the extent of A.

It will be seen that we thus treat the species as mutually exclusive, and very appropriately so, such mutual exclusiveness being their natural characteristic. When however we attempt to adapt this linear scheme to the more comprehensive case of alternatives which are not mutually exclusive, we soon find it fail us. *Two* non-exclusive alternatives indeed can be thus displayed, for such a scheme as the following will adequately mark the three

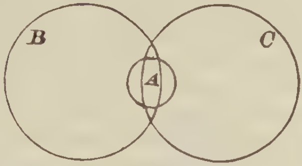


cases covered by "All A is either X or Y"; viz. that of A being X only, Y only, and both X and Y. But make the same attempt with three classes, X, Y, Z; and we see at once that it breaks down. We cannot possibly represent, by lines, the seven cases covered by "All A is either X or Y or Z": (If the reader will try he will find that no arrangement will yield more than six of the needed combinations unless we make one of the lines discontinuous, by breaking it into portions, which involves too violent a license to be permissible) and accordingly we should be forced to appeal to closed figures which possess greater capabilities in this respect.

Having just stated that Lambert has fairly represented a case of exclusive alternatives in disjunction, I must call attention to the fact that he expressly says that disjunctives do *not* admit of diagrammatic representation *. And his reason for so thinking deserves notice, as indicative of that deep distinction between the different accounts of the nature of propositions to which I have already had to allude. Starting with the assumption that B and C must be exclusive, he says that to represent 'A is either B or C' we may begin by drawing the lines for B and C beside one another, but then comes in the uncertainty that we do not know under which of the two we are to set the A line. We have grounded on a mere hypothetical and can get no further. What we are here doing is to regard A as a *Begriff* or concept, in which case it becomes a unity, and we are then naturally in uncertainty as to whether to refer it to B or C. The case is much the same as if we had to exhibit the individual disjunctive 'Socrates is either awake or asleep.' But interpret A in its class extent, and the disjunctive 'A is either B or C' becomes 'the classes B and C together make up A' and is essentially the same state of things as is readily represented by the subordination of species to a genus.

* "Die disjunctiven Sätze lassen sich gar nicht zeichnen, und zwar wiederum, weil sie nichts positives setzen." (*Neues Organon. Dian.* § 190.)

For the general representation of disjunctives on this plan, uler's circles do not answer much better than Lambert's lines. fact we cannot even represent 'All A is B or C (only)' by rcles, but are confined to 'All A is B or C (or both),' as thus :—



r if the B and C circles are not caused to intersect one another, e A circle will of course have to include something which lies tside them, and accordingly the point aimed at in the disjunction ils to be represented.

Kant (*Logik*, I. § 29) may be also noticed here as one of the ery few logicians who have given a diagram to illustrate disnctives. Like Lambert,—in fact like so many logicians,—he akes all disjunctives mutually exclusive. All he does indeed to take a square and divide it up into four smaller squares; ese four dividing members therefore just make up between em the whole sphere of the divided concept. So few however ave been the attempts to represent Disjunctives in Logic that it ems hardly worth while to pursue the subject any further.

Before quitting these historical points I may briefly notice application to which diagrammatic notation very readily lends self, but which seems to me none the less an abusive employent of it. I refer to the attempt to represent quantitatively e relative extent of the terms. When, for instance, we have rawn, either by lines or circles, a figure to represent 'All A is B' strikes us at once that we have got another element at our rvice; or, as a mathematician would say, there is still a disosable constant. We may draw the B circle or line of any size length we please; why not then so draw it as to represent e relative extension of the B class as compared with the A ass?

This idea seems to have occurred to logicians almost from the rst, as was indeed natural, considering that the use of diagrams as of course borrowed from mathematics, and that a clear oundary line was not always drawn between the two sciences. hus Lambert certainly seems to maintain that in strictness e must suppose each line to bear to any other the due proortionate length assigned by the extension of the terms. He ven recognizes the difficulty in the case of a single line, viz. as to

what length it should be drawn, resolving this however by the consideration that the unit of length being at our choice, any length will do if the unit be chosen accordingly. In the latter part of the *Neues Organon*,—where he is dealing with questions of Probability, and the numerically, or rather proportionately, definite syllogism,—the length of the lines which represent the extent of the concepts becomes very important. So little was he prepared to regard the diagram as referring solely to the purely logical considerations of presence and absence of class characteristics, of inclusion and exclusion of classes by one another.

Of course if considerations of this kind were to be taken into account it would follow almost necessarily that circles should be abandoned in the formation of our diagrams; since their relative magnitude, or rather the relative magnitude of the figures produced by their intersection, is not at all an easy matter of intuitive observation. We should be reduced to the choice of lines or parallelograms, so that the almost exclusive employment of Eulerian circles has caused this quantitative application to be much less adopted than would otherwise probably have been the case. This, however, is what has been done quite recently by F. A. Lange (*Logische Studien*), who in one of his Essays has made considerable use of diagrammatic methods in illustration of the Logic of Probability*. But I cannot regard the success of such a plan as encouraging. For the alternative forced upon us is this:—If we adhere to geometrical figures that are continuous, then the shapes of the various subdivisions soon become complicated; for, by the time we have reached four or even three terms, their combinations would result in yielding awkward compartments, whose relative areas could not be estimated intuitively. If, on the other hand, we take our stand on having ultimate compartments whose relative magnitudes admit of ready computation we are driven to abandon continuous figures. Our ABC compartment, say, instead of being enclosed in a ring fence is scattered about the field like an ill-arranged German principality of olden times, and its component portions require to be brought together in order to collect the whole before the eye. We draw a parallelogram to stand for A, and divide it into its B and not-B parts. If we divide each of these again into their C and not-C parts, we shall find that the B and not-B, the C and not-C compartments will not lie in juxtaposition with one another, and

* Every student of Probability is of course familiar enough with the converse case, viz. that of reducing spatial relations to symbolic statement. Whenever we compute the chance that a ball dropped at random upon a frame work will strike such and such a partition we are employing the same analogy as when we resort to diagrammatic representation of one of these quantitative logical propositions.

therefore the eye cannot conveniently gather them up into single groups. In fact such a plan almost necessarily leads to that primitive arrangement proposed by J. C. Lange, and mentioned at the outset of this paper. Whatever elegance logical diagrams can possess, and whatever aid they can give to the mind through the sense of sight, seem thus to be forfeited.

My own conviction is very decided that all introduction of considerations such as these should be avoided as tending to confound the domains of Logic and mathematics; of that which is, broadly speaking, qualitative, and that which is quantitative. The compartments yielded by our diagrams must be regarded solely in the light of being bounded by such and such figures, as lying inside or outside such and such lines. We must abstract entirely from all consideration of their relative magnitude, as we do of their actual shape, and trace no more connection between these facts and the logical extension of the terms which they represent than we do between this logical extension and the size and shape of the letter symbols, *A* and *B* and *C*.

On the beds at Headon Hill and Colwell Bay in the Isle of Wight. By E. TAWNEY, M.A., Trinity College, and H. KEEPING of the Woodwardian Museum.

The authors upheld the work of the Geological Survey, and of the late E. Forbes in this district, maintaining the correctness of the *Survey Memoirs* on the Isle of Wight and of their Horizontal and Vertical sections (except in the case of a few minor details). These however have been lately written against by Prof. Judd (*Quart. Journ. Geol. Soc.* XXXVI. p. 137), who disputes their identifications and stated succession of the beds in Totland and Colwell Bay; introducing two new series—a marine and a freshwater—at Headon Hill, in addition to those which have been universally accepted for the last quarter of a century.

Prof. Judd's section differs materially from that of the Geological Survey as regards the position of the marine series known as the "Middle Headon," or "Middle marine;" this series he places at the sea-level near Widdick Chine at the N.E. corner of Headon Hill. Consequently between the top of the marine bed and that of the Bembridge limestone, there would be on this theory 250 feet of beds, for such is the altitude of the cottage on the Warren marking the summit of the Bembridge limestone. The authors however dissent from this, and argue that the thickness stated must be reduced by about 105 feet; viz. the altitude of the top of the Middle Headon at this point: this 105 feet of beds—another freshwater and another marine—have no existence in fact, and in Prof. Judd's

section where they are represented, they can only have been obtained by counting the Lower and Middle Headon twice over.

The proof of the correctness of the description by the Survey of the beds at this point in opposition to the views of Prof. Judd has been worked out by a fresh measurement in detail in a vertical direction of all the strata between the Bembridge limestone and the sea-level. The strata were measured bed by bed, the fossils in each being noted, so that the limits of the freshwater and marine might be determined, and the various members be recognized by the distribution of fossils as well as by lithological characters.

The beds at this point with their thickness are in abstract as follows—in descending order:—

Bembridge limestone	25 feet,
Osborne Marls........................	70 feet,
Upper Headon	51 feet,
Middle „ (marine)	33 feet,
Lower „ to sea-level	71 feet.

The only marine beds are those of the Middle Headon, enclosed between the altitudes of 70 feet and 105 feet above sea-level; the remainder are all freshwater. The description of these and their gross thickness agrees fairly well with the vertical sections of the Geological Survey, and is held to be corroborative of their results.

The point then in which Prof. Judd's section differs from the above, lies in this, that a second marine series which he terms "Brockenhurst series" with another freshwater below it, in all 105 feet, is intercalated above the Upper Headon, these new formations having that portion of the section allotted to them which is occupied by the freshwater Osborne Marls, and part of the Upper Headon in the above section. No positive evidence is adduced by Prof. Judd of the existence of this second marine series at the spot where the Survey place the Osborne Marls; it is apparently supposed to be concealed by gravel and landslips. The authors have however examined the ground, and they find no gravel or concealment of beds at the place indicated, but recognize there the red and greenish mottled Osborne Marls, precisely as laid down in the vertical sections of the Survey. They therefore hold that no second marine series of any kind exists at this spot, much less any bed having the peculiar fauna of the Brockenhurst bed, which does not occur at all at the west end of the island; moreover, if it did, it would be found at the base of the Middle Headon, and not above the Upper Headon, where it is supposed to be by Prof. Judd. The authors object therefore to the cor-

relation of the Brockenhurst with the Colwell Bay bed, and they proceed to show that the marine Colwell bed is—as has been universally admitted for the last 25 years—identical with the marine (Middle Headon) bed of Headon Hill, whereas Prof. Judd seems to have separated them by 105 feet of hypothetical strata.

In establishing this point, attention is paid to the correlation of the various Lymnæa limestones which occur in the freshwater Headon series, a point of some confusion among previous writers. It is shown that the thick limestone in the Upper Headon of Headon Hill is at a considerably higher stratigraphical level than the limestone which forms How Ledge—which latter exists at a lower level in Headon Hill being the top bed of the Lower Headon. The identification of this limestone would carry with it necessarily the identification of the marine series lying immediately above it. Accordingly a minute comparison is instituted between the beds which constitute the marine Middle Headon at Headon Hill with the marine series at Colwell Bay. As far as the general distribution of fossils in the whole is concerned they are identical, while a few special beds may also be recognized by similar lithological character and fossil contents as identical at both localities.

The Middle Headon is denuded away from the top of the cliffs in the centre of Totland Bay, between Weston and Widdick Chines*, the last place where they are seen in Warden Cliff being close to the flagstaff of the Coast-Guard Station. An examination of the series there shows that they may be identified, bed by bed, with the Middle Headon of Headon Hill, while the beds of Warden Cliff are visibly and absolutely continuous with those of Colwell Bay.

This has been the accepted view for the last half century as far as the marine series of Colwell Bay is concerned, though it differs widely from that expressed by Prof. Judd. The Middle Headon having been followed through the cliffs, the same method of observation is applied to the freshwater Lower Headon. Those at Widdick Chine display certain beds: e.g. the Unio-bed, five thin Lynnæa and Chara limestones, which are traceable more or less through this portion of the cliff up to Weston Chine, and so join on to the better exposed Warden cliff beds. Thus the continuity of the Lower Headon beds which occupy all the cliff between the two Chines is easily followed, and the beds of Warden Cliff are identified with those of Headon Hill without a shadow of a doubt.

* The authors are indebted to the Rev. O. Fisher, M.A., for the information that he has this autumn discovered the marine Middle Headon in the Totland Bay brickyard which lies a little above and inland of this portion of the cliff, thereby proving that the bed was continuous above the top of the cliff, and linking the Warden Cliff exposure to that of Headon Hill.

To complete the section, the details of the Upper Headon at Cliff End are described for comparison with this series in Headon Hill; the thick limestone has thinned from 27 feet to about 2 feet, which is counterbalanced by an increased thickness in the sands. The Osborne Marls succeed having the same characters as at Headon Hill.

Palæontological evidence. Exception is taken to Prof. Judd's method of mixing fossils from various localities in the same list; thus he places the fossils from the Colwell Bay, Brockenhurst, and White Cliff Bay marine series in one list, as if these were all from the same horizon, or of the same age, and compares them with another list, containing the fauna of Headon Hill and Hordwell marine—also mingled together,—and from this comparison he finds that less than one half of the fossils in the latter two occur in the former three places, from which it is assumed that the Colwell Bay and Brockenhurst beds are of not the same age as those of Headon Hill—they are conceived to be newer.

The authors point out that it amounts to begging the question to assume the equivalence of the Colwell Bay and Brockenhurst beds—that is one of the points to be settled—and that nothing can be proved from a list in which fossils from the various localities are commingled.

Accordingly in the Tables prepared by the authors, the fossils from each locality are given in a separate column, so as to be easily comparable.

A separate list is however given of forms, which they have collected this year from the Middle Headon of Colwell Bay and Headon Hill, everything in the latter list having been found by their own hands; out of 57 species at Colwell Bay, they found 53 in the middle marine of Headon Hill, a proportion of about 93 per cent.; even of the 4 which are missing most have been found previously. These figures are held to prove the identity of the horizon in the two localities.

The distribution of one or two leading fossils is then discussed. Prof. Judd has argued that the Headon Hill marine bed is not equivalent to that of Colwell Bay, because it contains *Cerithium ventricosum* and *C. concavum*, which, he says, do not occur at Colwell Bay. The authors show that is an error. They point out the position of the one bed at Headon Hill, in which *C. ventricosum* abounds, and find it occupying precisely an analogous position at Colwell Bay. *C. concavum* has a wider range at Headon Hill, where it is very abundant, since it occurs through the greater part of the Middle Headon there; the authors have found it however in the "Venus-bed" of Colwell Bay. The distribution of these and other fossils show that the palæontological evidence is in

harmony with the stratigraphical. They arrive therefore at the conclusion—the one universally accepted hitherto—that there is only one marine series in this section, viz. the Middle Headon of E. Forbes enclosed between the freshwater Lower and Upper Headon, while the Brockenhurst bed does not occur anywhere in the West of the island.

White Cliff Bay and the New Forest. Professor Judd's reading of the White Cliff Bay section (at the East end of the island) is next criticised. It is pointed out that to rename the whole of what has been hitherto known as Middle Headon, and call it "Brockenhurst series," is quite contrary to the evidence of fossil distribution.

The Brockenhurst bed has been recognised by the Rev. O. Fisher and other writers so long ago as 1864, as existent in the lowest two feet of the Middle Headon in the Vertical section of White Cliff Bay, published by the Geological Survey. This bed is extraordinarily rich in fossils like its German congener, while many of them are peculiar to it. In a few days work, the authors collected 70 species from this one bed, all of them identical with those of the well-known bed in the railway cutting near Brockenhurst; many of these have been found at no other horizon, and do not pass up into the Venus-bed. The Survey have been perfectly correct in the position assigned to the latter bed at White Cliff Bay; it will be observed that it forms the upper part of the Middle Headon there, and lies above the bed in which the Brockenhurst fauna was afterwards recognized, being separated from it by yellow sands, &c. These lower zones are not developed at the West end of the island, the Middle Headon at Totland Bay being less complete there. The "Brockenhurst zone" is restricted here to the lower 2 feet of the Middle Headon, and it lies immediately on the eroded surface of the Lower Headon. The authors propose to designate as "Roydon Zone" the greenish gray and yellow sands which intervene between it and the Venus-bed.

In the New Forest section it is shown that Prof. Judd has reversed the order of the beds by placing the Brockenhurst series above the marine Middle Headon: this is plainly at variance with obvious facts both at Brockenhurst and White Cliff Bay. The exposure of this misapprehension as to the position of the Brockenhurst bed in the series carries with it a refutation of his theory as to the existence of this bed high up in Headon Hill. It is not only not in existence there, but the theoretical position assigned to it in the Hill would be false.

Affinities of Brockenhurst fauna. Prof. Judd's statement is next examined, whether while "nearly one-third of the Hordwell and Headon Hill marine shells are Barton forms, not more than

one-fifth of those occurring at Brockenhurst, Colwell Bay, and White Cliff are found at Barton." It would be certainly anomalous if the Venus-bed had more Barton forms than the Brockenhurst one, seeing that the former occupies a higher zone in the Middle Headon series.

An analysis is made of Prof. Judd's lists from which it appears that in the one list are nine species said to pass into Barton beds, while in the other list, these identical species have that range denied to them. Again, in his Brockenhurst and Colwell Bay list are 22 species of which the range into Barton beds is not recognized, while an examination of the Edward's collection in the British Museum shows that they pass up from Barton or Bruckleshain beds.

From the authors' lists it appears, on the other hand, that the percentage of Barton forms in the Whitley Ridge bed is about 42 per cent., a lower proportion than at White Cliff, because of the number of corals special to the locality. At White Cliff Bay this bed has 52 per cent. The proportion of Barton forms from all the Brockenhurst localities, including the Roydon Zone, is 48 per cent. If for comparison the percentage of Barton forms in the Middle Headon of Headon Hill is calculated, it is found to be 29 per cent. Fossil evidence leads therefore to the conclusion that the Headon Hill marine bed is later in age than the Brockenhurst, the proportion of Barton forms in the latter being not one fifth, but nearly one half—a result in accordance with their stratigraphical position.

Similarly, to test by fossil evidence whether the Colwell Bay bed is nearer related to the Brockenhurst than is the Headon Hill one, the percentage of Barton forms is observed in each; in Colwell Bay, they were 29 per cent., in the Headon Hill bed also 29 per cent., while in the Brockenhurst bed they were 48 per cent.

In order to complete the proof from fossils, if any such were needed, it is noted that there are only two species in each case common to either Colwell Bay, or Headon Hill, and Brockenhurst, and not occurring at the other locality, while there are 26 species common to Colwell Bay and Headon Hill, and not occurring at Brockenhurst.

These results are in perfect accordance with the stratigraphical succession, and show that Prof. Judd has misconceived the position of the Brockenhurst bed. The authors therefore reject his proposed term of "Brockenhurst series," and revert to the classification and nomenclature of the Geological Survey.

PROCEEDINGS

OF THE

Cambridge Philosophical Society.

February 7, 1881.

PROFESSOR NEWTON, PRESIDENT, IN THE CHAIR.

Horace Darwin, M.A., Trinity College, was balloted for and duly elected a Fellow of the Society.

The following communication was made to the Society:

Determination of the greatest height consistent with stability that a vertical pole or mast can be made, and of the greatest height to which a tree of given proportions can grow. By A. G. GREENHILL, M.A.

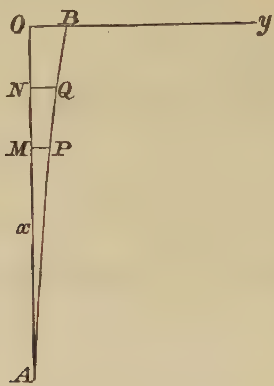
I. Suppose a uniform cylindrical pole or wire fixed in a vertical direction at its lowest point, and carried to such a height that the vertical position becomes unstable and flexure begins; it is required to determine this height.

Let $2a$ be the diameter in inches, and A the sectional area of the pole in square inches: and E be Young's modulus of elasticity of the substance, expressed in gravitation measure of ℔ to the square inch.

Then, if ρ be the radius of curvature of the central fibre of the pole, the bending moment of resilience (the unit being the inch-℔.)

$$= EI\frac{1}{\rho} = EAk^2\frac{1}{\rho}.$$

Take the origin O at the top of the pole in its vertical position and the axis Ox directed vertically downwards:



then if APB be the central line of the pole, supposed to be slightly curved under its own weight, we may put $\frac{1}{\rho} = \frac{dp}{dx}$, where $p = \frac{dy}{dx}$, and therefore the bending moment at P

$$= EAk^2 \frac{dp}{dx}.$$

This must be equated to the moment of the weight of PB about P, which

$$= wA \int_0^x (y' - y)\, dx',$$

x', y' denoting the co-ordinates of any point Q between B and P, and w the density of the substance in pounds to the cubic inch.

Therefore the differential equation of the central line APB is

$$EAk^2 \frac{dp}{dx} = wA \int_0^x (y' - y)\, dx',$$

or, differentiating with respect to x,

$$EAk^2 \frac{d^2p}{dx^2} = -wA \int_0^x p\, dx'$$

$$= -wAxp,$$

or $$x^2 \frac{d^2p}{dx^2} + \frac{w}{Ek^2} x^3 p = 0 \quad\quad (1).$$

To solve this differential equation, first put $p = x^{\frac{1}{2}}z$, then

$$x^2\frac{d^2z}{dx^2} + x\frac{dz}{dx} + \left(\frac{w}{Ek^2}x^3 - \tfrac{1}{4}\right)z = 0.$$

Again, put $x^3 = r^2$, and then

$$x^2\frac{d^2z}{dx^2} + x\frac{dz}{dx} = \tfrac{9}{4}\left(r^2\frac{d^2z}{dr^2} + r\frac{dz}{dr}\right),$$

and therefore

$$r^2\frac{d^2z}{dr^2} + r\frac{dz}{dr} + \left(\frac{4w}{9Ek^2}r^2 - \tfrac{1}{9}\right)z = 0 \text{..............}(2).$$

This is of the form of Bessel's differential equation

$$r^2\frac{d^2z}{dr^2} + r\frac{dz}{dr} + (\kappa^2r^2 - n^2)z = 0 \text{.................}(3),$$

where $$\kappa^2 = \frac{4w}{9Ek^2},\quad n^2 = \tfrac{1}{9}.$$

The solution of (3) is

$$z = AJ_n(\kappa r) + BJ_{-n}(\kappa r),$$

where $$J_n(x) = \frac{x^n}{\sqrt{\pi}2^n\Gamma(n+\frac{1}{2})}\int_0^\pi \cos(x\cos\phi)\sin^{2n}\phi\, d\phi.$$

(Todhunter, *Functions of Laplace, Lamé, and Bessel*, p. 285.)

Consequently the solution of (2) is

$$z = AJ_{\frac{1}{3}}(\kappa r) + BJ_{-\frac{1}{3}}(\kappa r),$$

and the solution of (1) is

$$p = \sqrt{x}\,\{AJ_{\frac{1}{3}}(\kappa x^{\frac{3}{2}}) + BJ_{-\frac{1}{3}}(\kappa x^{\frac{3}{2}})\}.$$

The condition that $\dfrac{dp}{dx} = 0$ when $x = 0$, makes $A = 0$, and then

$$p = B\sqrt{x}J_{-\frac{1}{3}}(\kappa x^{\frac{3}{2}}) \text{.........................}(4).$$

At A, the lowest point, we must have $p = 0$; and therefore, supposing the height OA to be h,

$$J_{-\frac{1}{3}}(\kappa h^{\frac{3}{2}}) = 0.$$

If c be the least root of the equation $J_{-\frac{1}{3}}(c) = 0$,

then $$c = \kappa h^{\frac{3}{2}},$$

$$h = \left(\frac{c}{\kappa}\right)^{\frac{2}{3}} = \left(\frac{9Ek^2c^2}{4w}\right)^{\frac{1}{3}},$$

the greatest height to which the pole can reach for the vertical position to be stable; if carried up to a greater height, the pole will curve under its own weight if slightly displaced.

From the expansion of $J_n(x)$ in a series of ascending powers of x,

$$J_{-\frac{1}{3}}(x) = \frac{x^{-\frac{1}{3}}}{2^{-\frac{1}{3}}\Gamma(\frac{2}{3})}\left(1 - \frac{3x^2}{2.4} + \frac{3^2x^4}{2.4.4.10} - \frac{3^3x^6}{2.4.6.4.10.16} + \dots\right),$$

and we find by trial that $c = 1{\cdot}88$, $c^{\frac{2}{3}} = 1{\cdot}52$, and

$$h = 1{\cdot}52\left(\frac{9Ea^2}{16w}\right)^{\frac{1}{3}} = 1{\cdot}26\left(\frac{Ea^2}{w}\right)^{\frac{1}{3}}.$$

For instance, for a solid cylinder of pine,

$$E = 1500000, \text{ about};$$

$$w = \frac{{\cdot}6 \times 62{\cdot}5}{12^3} = \frac{37{\cdot}5}{12^3};$$

and if the diameter of the pole be six inches, $a = 3$, $k^2 = \frac{1}{4}a^2$, and therefore $h = 89{\cdot}45 \times 12$, and the height in feet is 89·45.

For a steel wire

$$E = 31000000,$$

$$w = \frac{7{\cdot}8 \times 62{\cdot}5}{12^3} = \frac{487{\cdot}5}{12^3};$$

and if the diameter of the wire be one-tenth of an inch, $a = \frac{1}{20}$,

and
$$h = 1{\cdot}26 \times 12\left(\frac{31000000}{487{\cdot}5 \times 400}\right)^{\frac{1}{3}}$$

$$= 6{\cdot}81 \times 12,$$

and the height in feet is 6·81.

If a load be concentrated at the top B, of weight equal to that of a length l of the shaft, then the differential equation of the central line becomes

$$EAk^2\frac{dp}{dx} = wA\int_0^x (y' - y)\,dx' + wAl\,(OB - y),$$

and differentiating

$$Ek^2\frac{d^2p}{dx^2} = -w\,(x + l)\,p,$$

the same differential equation as before, with $x + l$ for x.

This state of things will be represented in nature by an exogenous tree like the Areca Palm, growing in a cylindrical shaft, and having a cluster of leaves at the top.

II. Suppose the rod to taper uniformly up to a point, being a right circular cone of semi-vertical angle α; then at the point P the bending moment, supposing r the radius of the cross section of the rod at P,

$$\tfrac{1}{4}\pi E r^4 \frac{dp}{dx} = \pi w \int_0^x (y' - y) r'^2 dx',$$

and differentiating with respect to x,

$$\tfrac{1}{4}\pi E \frac{d}{dx}\left(r^4 \frac{dp}{dx}\right) = -\pi w p \int_0^x r'^2 dx'.$$

Now $r = x \tan \alpha$, and therefore

$$\tfrac{1}{4}\pi E \tan^4\alpha \frac{d}{dx}\left(x^4 \frac{dp}{dx}\right) = -\pi w p \tan^2\alpha \int_0^x x'^2 dx'$$

$$= -\tfrac{1}{3}\pi w \tan^2\alpha x^3 p,$$

or

$$x^2 \frac{d^2p}{dx^2} + 4x \frac{dp}{dx} + \frac{4w}{3E\tan^2\alpha} xp = 0 \quad\text{..................(1)},$$

the differential equation of the central line of the conical rod, the flexure under its own weight being small.

First put $p = x^{-\frac{3}{2}} z$, then

$$x^2 \frac{d^2z}{dx^2} + x \frac{dz}{dx} + \left(\frac{4w}{3E\tan^2\alpha} x - \tfrac{9}{4}\right) z = 0;$$

and again, putting $x = r^2$,

$$r^2 \frac{d^2z}{dr^2} + r \frac{dz}{dr} + \left(\frac{16w}{3E\tan^2\alpha} r^2 - 9\right) z = 0 \quad\text{............(2)},$$

Bessel's differential equation, in which

$$\kappa^2 = \frac{16w}{3E\tan^2\alpha}, \qquad n^2 = 9.$$

Therefore the solution of (1), subject to the condition that p is finite when $x = 0$, is

$$p = Ax^{-\frac{3}{2}} J_3(\kappa x^{\frac{1}{2}}),$$

and supposing h the height OA, and c the least root of the equation $J_3(x)=0$, then

$$c=\kappa h^{\frac{1}{2}},$$

or

$$h=\left(\frac{c}{\kappa}\right)^2,$$

$$=\frac{3E\tan^2\alpha c^2}{16w}.$$

The value of c for $n=3$ is 6·379 (Table B. p. 274, Rayleigh, *Theory of Sound*, Vol. I.), and therefore

$$h=7\cdot63\frac{E}{w}\tan^2\alpha.$$

For instance with pine, where

$$E=1500000, \text{ and } w=\frac{37\cdot5}{12^3},$$

$$h=527380000\tan^2\alpha;$$

and if b be the radius of the base in inches, $\tan\alpha=\frac{b}{h}$,

$$h^3=527380000b^2,$$

or

$$h=807\cdot9b^{\frac{2}{3}},$$

h and b being given in inches.

III. If the rod be in the form of a paraboloid of revolution, then the differential equation of the central line

$$\tfrac{1}{4}\pi E\frac{d}{dx}\left(r^4\frac{dp}{dx}\right)=-\pi wp\int_0^x r'^2dx',$$

becomes, since $r^2=4mx$, where $4m$ is the latus rectum of the generating parabola,

$$4\pi m^2E\frac{d}{dx}\left(x^2\frac{dp}{dx}\right)=-4\pi mwp\int_0^x x'dx'$$

$$=-2\pi mwx^2p,$$

or

$$x^2\frac{d^2p}{d^2x}+2x\frac{dp}{dx}+\frac{w}{2Em}x^2p=0,$$

or

$$x\frac{d^2p}{dx^2}+2\frac{dp}{dx}+\frac{w}{2Em}xp=0,$$

which may be written

$$\frac{d^2}{dx^2}(xp) + \frac{w}{2Em}(xp) = 0 \text{..................(1)};$$

the solution of which is, subject to the conditions that p is finite and $\frac{dp}{dx} = 0$ when $x = 0$,

$$xp = A \sin \sqrt{\left(\frac{w}{2Em}\right)} x \text{........................(2)};$$

and therefore the height h is given by

$$\sin \sqrt{\left(\frac{w}{2Em}\right)} h = 0,$$

or

$$\sqrt{\left(\frac{w}{2Em}\right)} h = \pi,$$

or

$$h = \pi \sqrt{\left(\frac{2Em}{w}\right)}.$$

If b be the radius of the base, then $b^2 = 4mh$, or

$$h = \pi \sqrt{\left(\frac{E}{2wh}\right)} b,$$

or

$$h^{\frac{3}{2}} = \pi \sqrt{\left(\frac{E}{2w}\right)} b.$$

If k denote the elasticity of volume, and n the elasticity of figure, or as it is called, the rigidity of the substance, then

$$E = \frac{9nk}{3k + n}$$

(Thomson and Tait, *Natural Philosophy*, p. 521); and for a substance like jelly, n is small compared with k, and we may put $E = 3n$.

Now E and n being expressed in gravitation measure, the rate of propagation v of transversal vibrations is given by

$$v^2 = g\frac{n}{w} = \frac{Eg}{3w},$$

and therefore

$$h^{\frac{3}{2}} = \pi \sqrt{\frac{3}{2g}} \cdot vb,$$

from which the greatest height a jelly in the form of a paraboloid may be made can be inferred.

IV. Generally, if we have a solid of revolution like a tree, of which the radius of the section of the trunk at the depth x below the top is r, and if W be the weight in pounds of the part of the tree above this section, then if the tree be of such a height as to be slightly bent under its own weight, the differential equation of the central line of the tree is, as before,

$$\tfrac{1}{4}\pi E r^4 \frac{dp}{dx} = \int_0^x (y' - y) \frac{dW'}{dx'} dx',$$

and differentiating with respect to x,

$$\tfrac{1}{4}\pi E \frac{d}{dx}\left(r^4 \frac{dp}{dx}\right) = -p \int_0^x \frac{dW'}{dx'} dx' = -Wp \text{ .... (1).}$$

This differential equation is equivalent to Bessel's differential equation if r and W are proportional to powers of x.

For suppose $r = \lambda x^m$, $W = \mu x^n$; then

$$\tfrac{1}{4}\pi E \lambda^4 \frac{d}{dx}\left(x^{4m} \frac{dp}{dx}\right) + \mu x^n p = 0,$$

or

$$x^{4m} \frac{d^2p}{dx^2} + 4m x^{4m-1} \frac{dp}{dx} + \frac{4\mu}{\pi E \lambda^4} x^n p = 0,$$

or

$$x^2 \frac{d^2p}{dx^2} + 4mx \frac{dp}{dx} + \frac{4\mu}{\pi E \lambda^4} x^{n-4m+2} p = 0 \text{............(2),}$$

which is reduced to Bessel's differential equation by putting

$$p = x^{\frac{1-4m}{2}} z, \text{ and } x^{n-4m+2} = r^2.$$

For instance, in I. $m = 0$, $n = 1$; in II. $m = 1$, $n = 3$;

in III. $m = \frac{1}{2}$, $n = 2$.

The solution of (2), subject to the condition that p is finite or $\dfrac{dp}{dx} = 0$ when $x = 0$, is as before

$$p = Ax^{\frac{1-4m}{2}} J_{\frac{4m-1}{n-4m+2}}\left(\kappa x^{\frac{n-4m+2}{2}}\right),$$

where

$$\kappa^2 = \frac{16\mu}{\pi E \lambda^4 (n - 4m + 2)^2};$$

and the greatest height h to which the tree can grow without flexure is given by

$$c = \kappa h^{\frac{n-4m+2}{2}},$$

or

$$h = \left(\frac{c}{\kappa}\right)^{\frac{2}{n-4m+2}},$$

where c is the least positive root of the equation

$$J_{\frac{4m-1}{n-4m+2}}(x) = 0.$$

By assigning different values to m and n according to the growth of the tree, and to E, μ, and λ according to the elasticity and density of the wood, the greatest straight vertical growth of a tree can be inferred. The application of these formulæ to the case of the large trees of California would be interesting, but in the absence of the numerical data required, I am unable to carry this out.

This paper was written for Dr Asa Gray, Professor of Botany in Harvard University, Cambridge, Mass., and was to have been read at the meeting of the American Association last year, but arrived too late.

A pine tree, as described in Sproat's *British Columbia* (1875), is said to have grown in one straight tapering stem to a height of 221 feet, and to have measured only 20 inches in diameter at the base.

Considered as an example of Article II., and neglecting the weight of the branches, a diameter of 20 inches at the base would allow of a vertical stable growth of about 300 feet.

Perhaps the best assumptions to make for our purpose as to the growth of a tree are, (i) to assume a uniformly tapering trunk as a central column, and (ii) to adopt Ruskin's assumption (*Modern Painters*), that the sectional area of the branches of a tree, made by any horizontal plane, is constant.

This is equivalent to putting $m=1$ and $n=1$ in equation (2), and then the solution depends on the least root of the associated Bessel's function of the order -3.

Generally, for a homogeneous body, $n = 2m+1$, and the diameter at the base must increase as the $\frac{3}{2}$ power of the height, which accounts for the slender proportions of young trees, compared with the stunted appearance of very large trees.

February 21, 1881.

PROFESSOR NEWTON, PRESIDENT, IN THE CHAIR.

T. H. Corry, Gonville and Caius College, F. R. Weldon, St John's College, and W. Heap, were balloted for and duly elected associates of the Society.

The following communication was made to the Society:

On the estimation of ferment in gland-cells by means of osmic acid. By J. N. LANGLEY, M.A., Fellow of Trinity College.

A few years ago Nussbaum[1] observed that such ferments as he could obtain stained rapidly with osmic acid. In consequence of this observation he made many others upon the staining power with osmic acid of ferment-producing glands. He arrived at the conclusion that the depth of staining with osmic acid was a satisfactory indication of the ferment-content of gland-cells.

Mr Langley briefly reviewed the facts that have been brought forward for[2] and against[3] Nussbaum's method and conclusions. He contended:—

That the cells of different glands do not increase in staining power in proportion to the amount of ferment that can be obtained from them.

That the cells of any one gland do not increase and decrease in staining power as the cells increase and decrease in various physiological states in ferment-content.

That there is no obvious correspondence between the depth of staining with osmic acid of extracts of ferment-producing glands and the amount of ferment contained by the extracts. This point was illustrated by experiments.

From these facts it follows that the depth of staining with osmic acid of any gland-cell is *not* a satisfactory indication of the power of the cell to produce ferment.

There are very few physiological substances which, when isolated, reduce osmic acid readily. Those that have this action, such as bile acids, hæmoglobin and peptone, do not, as far as we know, occur in living gland-cells; this taken together with the fact that glandular tissues diminish in staining power when they

[1] Nussbaum, *Archiv f. Mik. Anat.* Bd. XIII. S. 746, 1877.

[2] Nussbaum, *Archiv f. Mik. Anat.* Bd. XV. S. 119, 1878; Bd. XVI. S. 543, 1879. Edinger, *Archiv f. Mik. Anat.* Bd. XVII. S. 193, 1879.

[3] Grützner. *Pflüger's Archiv*, Bd. XVI. S. 122, 1877; Bd. XX. S. 395, 1879. Langley, *Foster's Jour. Physiol.* Vol. II. p. 271, 1879.

are kept for some hours before being put in osmic acid suggests that the staining of the normal tissue depends upon the presence of some unstable constituent of the living protoplasm not at present isolated.

March 7, 1881.

PROFESSOR NEWTON, PRESIDENT, IN THE CHAIR.

The following communications were made to the Society:

(1) *On the Action of the Vagus Nerve upon the frog's heart.* By W. H. GASKELL, M.D., Trinity College.

The chief object of this communication was to demonstrate by means of curves certain effects of vagus stimulation as obtained by a new method.

The method used was the following:

The heart was cut out, with vagus attached, the bulbus aortæ held tightly by means of Kronecker's forceps, the ventricle slit open and the extreme apex attached by a thread to an ordinary lever, so that the strip of cardiac tissue between the bulbus and the apex alone moved the lever and recorded its movements on the blackened drum.

Also simultaneous tracings of the beats of the auricles and ventricle and of the base and apex of the ventricle were obtained by using an upper and lower lever and clamping, not too tightly, the heart, either between the auricles and ventricle or midway between the base and apex of the ventricle. The curves obtained by these methods demonstrated the following facts:

1. Weak stimulation of the vagus causes simply a marked increase in the force of the contractions of the ventricle with, as a rule, a slight acceleration in the rate of beat.

2. Stronger stimulation causes a diminution in the force of the contractions followed by an increase; both during and after the stimulation the rate of rhythm is frequently increased.

3. When strong stimulation causes a complete standstill the ventricular contractions increase in force and rapidity beyond the normal after the stimulation has ended.

4. The stimulation of the nerve may cause steady increase in the force of the auricle contractions with simultaneous diminution even to complete standstill of the ventricle contractions. After the stimulation the increase in the ventricle contractions occurs when the auricular have already begun to diminish.

5. When the ventricle is beating alternately weakly and strongly, stimulation of the nerve makes all the beats equally strong.

6. This alternation of beats may occur in the apex of the ventricle alone while the base beats regularly, and then the vagus stimulation causes the apex beats to become equally strong.

Further by the method used the action of poisons, such as Atropin, Muscarin, Digitalin, on different parts of the heart and on the effect of the vagus nerve was demonstrated, and it was shewn that:

1. When the beats of the heart were reduced in size as by the application of normal saline solution, then Atropin Sulphate 1 p. c. solution applied to the heart caused an increase in the force of the contractions with a slower rhythm.

2. Atropin applied to the heart caused a slower rhythm with strong contractions and removed all the various effects of vagus stimulation.

3. Atropin applied to the ventricle only did not prevent the action of the nerve on the ventricle.

4. Atropin applied to the auricles and sinus only removed the whole effect of the nerve from all parts of the heart.

5. Muscarin applied to the ventricle only reduced the size of its contractions and relaxed the tissue without altering the rhythm at all events at first, and without affecting the auricles in the least.

6. When after the application of Muscarin or Digitalin the ventricle had ceased beating in a relaxed or semi-contracted condition respectively, then Atropin in each case brought back the beats without altering the condition of the tissue.

The author suggested from the consideration of these and many other curves which he possessed, that a possible explanation of the action of the vagus might be found on the hypothesis that the vagus is the trophic nerve of the cardiac muscle. He however could not at that stage of his investigation give any definite explanation of the phenomena observed by him, but trusted that further experiments would enable him to do so.

(2) *On the ancestral form of the Chordata.* By F. M. BALFOUR, M.A., F.R.S., *Fellow of Trinity College.*

March 21, 1881.

PROFESSOR NEWTON, PRESIDENT, IN THE CHAIR.

Dr Armistead, Dr G. M. Bacon, Mr H. Baxter, Mr R. Bowes, Mr Bumpstead, Mr J. Carter, Mr A. Deck, Mr H. Gotobed, Mr A. Graham, Mr Marshall (Ely), Mr W. E. Pain, Mr A. Schuster, and Mr W. W. Smith, were balloted for and re-elected associates of the Society.

The President stated that it was with very great pleasure that he was enabled to place upon the table the charter of the Society, which had been out of their possession since 1852; and he was glad to think that the remarks made by him at the Annual Meeting on October 25 had led to its recovery. They were much indebted to Professor Paget and Messrs. S. and W. Peed for the assistance they had rendered, and the Council had passed a cordial vote of thanks to them for their services.

The following communications were made to the Society:

(1) *On Conjugate Functions of Cartesians and other Quartics.* By A. G. GREENHILL, M.A.

1. The well-known case of the conjugate functions of confocal ellipses and hyperbolas in which $x+iy$ is a trigonometrical function of $\xi+i\eta$ may be considered as derived from the integral

$$\int\frac{dz}{\sqrt{(z\,.\,1-z)}}=2u,$$

from which
$$z=\sin^2 u,$$
or
$$x+iy=\sin^2(\xi+i\eta),$$
and the foci of the system are at $z=0$ and $z=1$.

If r, r' denote the distances of a point from the foci, then

$$r^2=(x+iy)(x-iy),$$
$$=\sin^2(\xi+i\eta)\sin^2(\xi-i\eta),$$
or
$$r=\sin(\xi+i\eta)\sin(\xi-i\eta);$$
and
$$r'^2=(x-1+iy)(x-1-iy);$$
$$r'=\cos(\xi+i\eta)\cos(\xi-i\eta);$$

and therefore

$$r' + r = \cos 2i\eta = \cosh 2\eta,$$

$$r' - r = \cos 2\xi;$$

giving the confocal ellipses and hyperbolas.

The Jacobian of the system $\dfrac{d(x, y)}{d(\xi, \eta)} = f'(\xi + i\eta) f'(\xi - i\eta)$ if $z = f(\xi + i\eta)$, and is therefore equal to

$$4 \sin(\xi + i\eta) \cos(\xi + i\eta) \sin(\xi - i\eta) \cos(\xi - i\eta) \text{ or } 4rr'.$$

2. Now if we consider the integral

$$\int \frac{dz}{\sqrt{(z \,.\, 1 - z \,.\, 1 - k^2 z)}} = 2u,$$

then

$$z = \operatorname{sn}^2 u,$$

or

$$x + iy = \operatorname{sn}^2(\xi + i\eta);$$

and if r, r', r'' denote the distances of a point from the three foci $z = 0$, 1 and $\dfrac{1}{k^2}$, then

$$r = \operatorname{sn}(\xi + i\eta) \operatorname{sn}(\xi - i\eta),$$

$$r' = \operatorname{cn}(\xi + i\eta) \operatorname{cn}(\xi - i\eta),$$

$$r'' = \frac{1}{k^2} \operatorname{dn}(\xi + i\eta) \operatorname{dn}(\xi - i\eta);$$

and we easily obtain

$$r' - r \operatorname{dn} 2\xi = \operatorname{cn} 2\xi,$$

$$r' \operatorname{cn}(2\eta, k') + r \operatorname{dn}(2\eta, k') = 1;$$

so that the curves $\xi = \text{constant}$, and $\eta = \text{constant}$ are confocal Cartesians, cutting at right angles by reason of the fundamental property of conjugate functions; which is Prof. Crofton's theorem.

For

$$r = \operatorname{sn}(\xi + i\eta) \operatorname{sn}(\xi - i\eta),$$

$$= \frac{\operatorname{cn} 2i\eta - \operatorname{cn} 2\xi}{\operatorname{dn} 2i\eta + \operatorname{dn} 2\xi}$$

$$= \frac{1}{k^2} \frac{\operatorname{dn} 2i\eta - \operatorname{dn} 2\xi}{\operatorname{cn} 2i\eta + \operatorname{cn} 2\xi};$$

$$r' = \text{cn}\,(\xi + i\eta)\,\text{cn}\,(\xi - i\eta)$$
$$= \frac{\text{cn}\,2i\eta\,\text{dn}\,2\xi + \text{cn}\,2\xi\,\text{dn}\,2i\eta}{\text{dn}\,2i\eta + \text{dn}\,2\xi}$$
$$= \frac{k'^2}{k^2}\,\frac{\text{dn}\,2i\eta - \text{dn}\,2\xi}{\text{cn}\,2i\eta\,\text{dn}\,2\xi - \text{cn}\,2\xi\,\text{dn}\,2i\eta};$$
$$r'' = \frac{1}{k^2}\,\text{dn}\,(\xi + i\eta)\,\text{dn}\,(\xi - i\eta)$$
$$= \frac{1}{k^2}\,\frac{\text{cn}\,2i\eta\,\text{dn}\,2\xi + \text{cn}\,2\xi\,\text{dn}\,2i\eta}{\text{cn}\,2i\eta + \text{cn}\,2\xi}$$
$$= \frac{k'^2}{k^2}\,\frac{\text{cn}\,2i\eta - \text{cn}\,2\xi}{\text{cn}\,2i\eta\,\text{dn}\,2\xi - \text{cn}\,2\xi\,\text{dn}\,2i\eta};$$

and from these conditions

$$\left.\begin{aligned} r' - r\,\text{dn}\,2\xi &= \text{cn}\,2\xi \\ r' + r\,\text{dn}\,2i\eta &= \text{cn}\,2i\eta \end{aligned}\right\},$$

or

$$\left.\begin{aligned} r'' - r\,\text{cn}\,2\xi &= \frac{1}{k^2}\,\text{dn}\,2\xi \\ r'' + r\,\text{cn}\,2i\eta &= \frac{1}{k^2}\,\text{dn}\,2i\eta \end{aligned}\right\}, \qquad \text{(A)}$$

or

$$\left.\begin{aligned} r''\,\text{dn}\,2\xi - r'\,\text{cn}\,2\xi &= \frac{k'^2}{k^2} \\ r''\,\text{dn}\,2i\eta - r'\,\text{cn}\,2i\eta &= \frac{k'^2}{k^2} \end{aligned}\right\},$$

or

$$\left.\begin{aligned} k'^2 r - r'\,\text{dn}\,2\xi + k^2 r''\,\text{cn}\,2\xi &= 0 \\ -k'^2 r - r'\,\text{dn}\,2i\eta + k^2 r''\,\text{cn}\,2i\eta &= 0 \end{aligned}\right\},$$

the vectorial equations of one and the same system of confocal Cartesians.

Since

$$\frac{dz}{du} = 2\,\text{sn}\,u\,\text{cn}\,u\,\text{dn}\,u,$$

therefore if J denote the Jacobian, $\dfrac{d\,(x, y)}{d\,(\xi, \eta)}$,

$$J = 4\,\text{sn}\,(\xi + i\eta)\,\text{cn}\,(\xi + i\eta)\,\text{dn}\,(\xi + i\eta)$$
$$\text{sn}\,(\xi - i\eta)\,\text{cn}\,(\xi - i\eta)\,\text{dn}\,(\xi - i\eta)$$
$$= 4k^2\rho\rho'\rho''.$$

As explained in Maxwell's *Electricity*, chapter XII., ξ and η may be taken to denote equipotential cylindrical surfaces and lines of force, and then $J^{-\frac{1}{2}}$ will represent the electrification at any point.

Thus a cylinder whose cross section is a Cartesian will if electrified have a superficial density inversely proportional to the square root of the product of the distances from the three foci; the law for confocal conics being a particular case of this.

3. The theory of quartics with four collinear foci A, B, C, D may be considered as derived from the integral

$$\int \frac{dz}{\sqrt{(z-a\,.\,z-b\,.\,z-c\,.\,z-d)}},$$

and putting this

$$=\frac{2u}{\sqrt{(a-c\,.\,b-d)}},$$

then, using a notation employed by Clifford (*Proceedings of the London Mathematical Society*, Vol. VII. p. 29),

$$u = \mathrm{sn}^{-1}\sqrt{\frac{z-a}{z-b}\,\frac{b-d}{a-d}}$$

$$= \mathrm{cn}^{-1}\sqrt{\frac{z-d}{z-b}\,\frac{a-b}{a-d}}$$

$$= \mathrm{dn}^{-1}\sqrt{\frac{z-c}{z-b}\,\frac{a-b}{a-c}},$$

where

$$k^2 = \frac{b-c}{a-c}\,\frac{a-d}{b-d},$$

the anharmonic ratio of the four points A, D, C, B; or

$$\frac{z-a}{z-b}\,\frac{b-d}{a-d} = \mathrm{sn}^2 u,$$

$$\frac{z-d}{z-b}\,\frac{a-b}{a-d} = \mathrm{cn}^2 u,$$

$$\frac{z-c}{z-b}\,\frac{a-b}{a-c} = \mathrm{dn}^2 u,$$

and putting

$$z = x + iy,$$

$$u = \xi + i\eta,$$

and denoting by r_1, r_2, r_3, r_4 the distances of a point from the four foci A, B, C, D, given by $z = a, b, c, d$; then

$$\frac{r_1}{r_2}\frac{b-d}{a-d} = \operatorname{sn}(\xi + i\eta)\operatorname{sn}(\xi - i\eta),$$

$$\frac{r_4}{r_2}\frac{a-b}{a-d} = \operatorname{cn}(\xi + i\eta)\operatorname{cn}(\xi - i\eta),$$

$$\frac{r_3}{r_2}\frac{a-b}{a-c} = \operatorname{dn}(\xi + i\eta)\operatorname{dn}(\xi - i\eta);$$

and consequently, writing in equations (A), $\frac{r_1}{r_2}\frac{b-d}{c-d}$ for r, $\frac{r_4}{r_2}\frac{a-b}{a-d}$ for r', and

$$\frac{1}{k^2}\frac{r_3}{r_2}\frac{a-b}{a-c} \text{ or } \frac{r_3}{r_2}\frac{a-b}{a-d}\frac{b-d}{b-c} \text{ for } r'',$$

the vectorial equations of a system of quartics with four collinear foci may be written

$$\left.\begin{aligned} r_4(a-b) - r_1(b-d)\operatorname{dn} 2\xi &= r_2(a-d)\operatorname{cn} 2\xi \\ r_4(a-b) + r_1(b-d)\operatorname{dn} 2i\eta &= r_2(a-d)\operatorname{cn} 2i\eta \end{aligned}\right\},$$

with similar expressions connecting

$$r_1, r_2, r_3;\ r_1, r_3, r_4 \text{ and } r_2, r_3, r_4.$$

If $a = b$ and $c = d$, then

$$z - \tfrac{1}{2}(a+c) = \tfrac{1}{2}(a-c)\coth\tfrac{1}{2}u,$$

giving the dipolar system of circles.

4. The integral in its canonical form

$$\int \frac{dz}{\sqrt{(1-z^2 \,.\, 1-k^2z^2)}} = u,$$

gives $$z = \operatorname{sn} u,$$

or $$x + iy = \operatorname{sn}(\xi + i\eta);$$

and the four foci taken in order are given by

$$z = \frac{1}{k},\quad 1,\quad -1,\quad -\frac{1}{k}.$$

Therefore r_1, r_2, r_3, r_4, r denoting the distances of a point from the foci and from the origin,

$$r^2 = \text{sn}\,(\xi + i\eta)\,\text{sn}\,(\xi - i\eta),$$

$$r_2 r_3 = \text{cn}\,(\xi + i\eta)\,\text{cn}\,(\xi - i\eta),$$

$$r_1 r_4 = \frac{1}{k^2}\,\text{dn}\,(\xi + i\eta)\,\text{dn}\,(\xi - i\eta);$$

and therefore writing in equations (A) r^2 for r, $r_2 r_3$ for r', and $r_1 r_4$ for r'', then

$$\left.\begin{aligned} r_2 r_3 - r^2\,\text{dn}\,2\xi &= \text{cn}\,2\xi \\ r_2 r_3 + r^2\,\text{dn}\,2i\eta &= \text{cn}\,2i\eta \end{aligned}\right\},$$

or

$$\left.\begin{aligned} r_1 r_4 - r^2\,\text{cn}\,2\xi &= \frac{1}{k^2}\,\text{dn}\,2\xi \\ r_1 r_4 + r^2\,\text{cn}\,2i\eta &= \frac{1}{k^2}\,\text{dn}\,2i\eta \end{aligned}\right\},$$

or

$$\left.\begin{aligned} r_1 r_4\,\text{dn}\,2\xi - r_2 r_3\,\text{cn}\,2\xi &= \frac{k'^2}{k^2} \\ r_1 r_4\,\text{dn}\,2i\eta - r_2 r_3\,\text{cn}\,2i\eta &= \frac{k'^2}{k^2} \end{aligned}\right\},$$

represent one and the same system of orthogonal quartic curves.

5. For $\quad x + iy = \text{cn}\,(\xi + i\eta),$

the foci are given in order by

$$z = 1, \quad i\frac{k'}{k}, \quad -1, \quad -i\frac{k'}{k};$$

and

$$r^2 = \text{cn}\,(\xi + i\eta)\,\text{cn}\,(\xi - i\eta),$$

$$r_1 r_3 = \text{sn}\,(\xi + i\eta)\,\text{sn}\,(\xi - i\eta),$$

$$r_2 r_4 = \frac{1}{k^2}\,\text{dn}\,(\xi + i\eta)\,\text{dn}\,(\xi - i\eta);$$

and therefore the quartic curves are given by

$$\left.\begin{aligned} r^2 - r_1 r_3\,\text{dn}\,2\xi &= \text{cn}\,2\xi \\ r^2 + r_1 r_3\,\text{dn}\,2i\eta &= \text{cn}\,2i\eta \end{aligned}\right\},$$

with similar expressions connecting

$$r^2 \text{ and } r_2 r_4, \quad r_1 r_3 \text{ and } r_2 r_4.$$

6. For
$$x + iy = \mathrm{dn}\,(\xi + i\eta),$$
the foci are given by
$$z = 1, \quad k', \quad -k', \quad -1\,;$$
and
$$r^2 = \mathrm{dn}\,(\xi + i\eta)\,\mathrm{dn}\,(\xi - i\eta),$$
$$r_1 r_4 = k^2\,\mathrm{sn}\,(\xi + i\eta)\,\mathrm{sn}\,(\xi - i\eta),$$
$$r_2 r_3 = k^2\,\mathrm{cn}\,(\xi + i\eta)\,\mathrm{cn}\,(\xi - i\eta)\,;$$
and therefore the quartic curves are given by
$$\left.\begin{aligned} r_2 r_3 - r_1 r_4\,\mathrm{dn}\,2\xi &= k^2\,\mathrm{cn}\,2\xi \\ r_2 r_3 + r_1 r_4\,\mathrm{dn}\,2i\eta &= k^2\,\mathrm{cn}\,2i\eta \end{aligned}\right\},$$
with similar expressions connecting
$$r^2 \text{ and } r_1 r_4, \quad r^2 \text{ and } r_2 r_3;$$
but these curves derived from
$$x + iy = \mathrm{dn}\,(\xi + i\eta)$$
are similar to those derived from
$$x + iy = \mathrm{sn}\,(\xi + i\eta).$$

These quartic curves obtained from
$$x + iy = \mathrm{sn}\,(\xi + i\eta) \ \text{ or } \ \mathrm{cn}\,(\xi + i\eta) \ \text{ or } \ \mathrm{dn}\,(\xi + i\eta)$$
have been considered by Siebeck in *Crelle*, 57, and figures given.

The physical problems in connexion with the surfaces generated by the revolution of these curves have been considered by Dr Albert Wängerin in the *Transactions of the Academy of Sciences of Göttingen*, 1875; and he introduces certain functions, analogous to the *toroidal functions* employed by Mr W. M. Hicks for the solution of corresponding problems for the anchor ring surface.

Siebeck uses as vectorial co-ordinates the sum and difference of the focal distances r_2 and r_3; and denoting them by S and D, then
$$r^2 = \tfrac{1}{4}(S^2 + D^2) - 1,$$
$$r_2 r_3 = \tfrac{1}{4}(S^2 - D^2),$$

and by substitution in the preceding equations his results may be obtained.

7. The integral

$$\int \frac{dz}{\sqrt{(1-z^2 \,.\, 1-k^2z^2)}} = u,$$

giving

$$z = \operatorname{sn} u,$$

may however be written

$$\int \frac{dz}{\sqrt{\left(z-\frac{1}{k} \,.\, z-1 \,.\, z+1 \,.\, z+\frac{1}{k}\right)}} = ku,$$

and may be considered as a particular case of

$$\int \frac{dz}{\sqrt{(z-a \,.\, z-b \,.\, z-c \,.\, z-d)}},$$

where

$$a=\frac{1}{k}, \quad b=1, \quad c=-1, \quad d=-\frac{1}{k};$$

and therefore

$$\frac{z-\frac{1}{k}}{z-1} = \frac{2}{1+k} \operatorname{sn}^2 \tfrac{1}{2}(1+k)u,$$

$$\frac{z+\frac{1}{k}}{z-1} = \frac{2}{1-k} \operatorname{cn}^2 \tfrac{1}{2}(1+k)u,$$

$$\frac{z+1}{z-1} = \frac{1+k}{1-k} \operatorname{dn}^2 \tfrac{1}{2}(1+k)u;$$

and if λ denote the new modulus,

$$\lambda^2 = \frac{2}{\frac{1}{k}+1} \, \frac{\frac{2}{k}}{1+\frac{1}{k}} = \frac{4k}{(1+k)^2},$$

equivalent to Landen's transformation.

Therefore, putting $\frac{1}{2}(1+k)u = \xi' + i\eta'$,

$$\frac{r_1}{r_2} = \frac{2}{1+k} \operatorname{sn}(\xi'+i\eta') \operatorname{sn}(\xi'-i\eta'),$$

$$\frac{r_4}{r_2} = \frac{2}{1-k} \operatorname{cn}(\xi'+i\eta') \operatorname{cn}(\xi'-i\eta'),$$

$$\frac{r_3}{r_2} = \frac{1+k}{1-k} \operatorname{dn}(\xi'+i\eta') \operatorname{dn}(\xi'-i\eta');$$

and therefore the quartic curves derived from

$$x+iy=\operatorname{sn}(\xi+i\eta)$$

may be also expressed by

$$\left.\begin{array}{l}(1-k)r_4-(1+k)r_1\operatorname{dn}\ 2\xi'=2r_2\operatorname{cn}2\xi'\\(1-k)r_4+(1+k)r_1\operatorname{dn}2i\eta'=2r_2\operatorname{cn}2i\eta'\end{array}\right\},$$

where $$\xi'+i\eta'=\tfrac{1}{2}(1+k)(\xi+i\eta);$$

with similar expressions connecting r_1, r_2, r_3; r_1, r_3, r_4 and r_2, r_3, r_4.

The curves obtained from $x+iy=\operatorname{dn}(\xi+i\eta)$ may be similarly expressed.

8. The integral

$$\int\frac{dz}{\sqrt{\{(z-m)^2+n^2.(z-m')^2+n'^2\}}},$$

written in the form

$$\int\frac{dz}{\sqrt{(z-m-in.z-m+in.z-m'+in'.z-m'-in')}},$$

when considered as a case of

$$\int\frac{dz}{\sqrt{(z-a.z-b.z-c.z-d)}},$$

by putting

$$a=m+in,$$
$$b=m-in,$$
$$c=m'-in',$$
$$d=m'+in',$$

will give ultimately in a real form the substitutions required for the reduction of the integral in general, and also the quartic curves whose foci A, B, C, D are given by

$$z=m+in,\quad m-in,\quad m'-in',\quad m'+in'.$$

For $$k^2=\frac{b-c}{a-c}\,\frac{a-d}{b-d}$$

$$=\frac{m-m'-i.n-n'}{m-m'+i.n+n'}\,\frac{m-m'+i.n-n'}{m-m'-i.n+n'}$$

$$=\frac{(m-m')^2+(n-n')^2}{(m-m')^2+(n+n')^2}=\frac{AD^2}{AC^2},$$

and $$k'^2 = \frac{4nn'}{(m-m')^2+(n+n')^2} = \frac{AB\,.\,CD}{AC^2}.$$

If $$x+iy=z, \quad x-iy=z';$$
$$\xi+i\eta=u, \quad \xi-i\eta=u';$$

then $$r_1^2 = z-a\,.\,z'-b,$$
$$r_2^2 = z-b\,.\,z'-a,$$
$$r_3^2 = z-c\,.\,z'-d,$$
$$r_4^2 = z-d\,.\,z'-c;$$

and we may put
$$\frac{z-a}{z-b}\,\frac{b-d}{a-d} = \operatorname{sn}^2 u,$$
$$\frac{z-d}{z-b}\,\frac{a-b}{a-d} = \operatorname{cn}^2 u,$$
$$\frac{z-c}{z-b}\,\frac{a-b}{a-c} = \operatorname{dn}^2 u;$$

and therefore, changing i into $-i$,
$$\frac{z'-b}{z'-a}\,\frac{a-c}{b-c} = \operatorname{sn}^2 u',$$
$$\frac{z'-c}{z'-a}\,\frac{b-a}{b-c} = \operatorname{cn}^2 u',$$
$$\frac{z'-d}{z'-a}\,\frac{b-a}{b-d} = \operatorname{dn}^2 u'.$$

Therefore $$\operatorname{sn}^2 u \operatorname{sn}^2 u' = \frac{r_1^2}{r_2^2}\,\frac{a-c}{b-c}\,\frac{b-d}{a-d}$$
$$= \frac{r_1^2}{k^2 r_2^2},$$
$$\operatorname{cn}^2 u \operatorname{cn}^2 u' = \frac{r_4^2}{r_2^2}\,\frac{b-a}{b-c}\,\frac{a-b}{a-d}$$
$$= \frac{r_4^2}{r_2^2}\,\frac{4n^2}{(m-m')^2+(n-n')^2}$$
$$= \frac{n}{n'}\,\frac{r_4^2}{r_2^2}\,\frac{k'^2}{k^2},$$

$$\mathrm{dn}^2 u \,\mathrm{dn}^2 u' = \frac{r_3^{\,2}}{r_2^{\,2}} \frac{4n^2}{(m-m')^2+(n+n')^2}$$

$$= \frac{n}{n'} \frac{r_3^{\,2}}{r_2^{\,2}} k'^2;$$

or

$$\mathrm{sn}\, u \,\mathrm{sn}\, u' = \frac{r_1}{kr_2},$$

$$\mathrm{cn}\, u \,\mathrm{cn}\, u' = \frac{k'r_4}{kr_2}\sqrt{\frac{n}{n'}},$$

$$\mathrm{dn}\, u \,\mathrm{dn}\, u' = \frac{k'r_3}{r_2}\sqrt{\frac{n}{n'}};$$

and therefore

$$\left.\begin{array}{l} k'r_4\sqrt{\dfrac{n}{n'}} - r_1\,\mathrm{dn}\, 2\xi = kr_2\,\mathrm{cn}\, 2\xi \\ k'r_4\sqrt{\dfrac{n}{n'}} + r_1\,\mathrm{dn}\, 2i\eta = kr_2\,\mathrm{cn}\, 2i\eta \end{array}\right\},$$

with similar expressions connecting r_1, r_2, r_3 and r_2, r_3, r_4, are the vectorial equations of orthogonal quartic curves, with foci whose co-ordinates are

$$(m, \pm n)\;(m', \pm n').$$

9. Generally, if a, b, c, d be the vectors of the four foci A, B, C, D, then in the integral

$$\int \frac{dz}{\sqrt{(z-a\,.\,z-b\,.\,z-c\,.\,z-d)}},$$

the modulus of the elliptic functions will be real, when

$$\frac{b-c}{a-c}\,\frac{a-d}{b-d}$$

is real; that is when the angle between the vectors $\overline{AD}$ and $\overline{DB}$ is equal to the angle between the vectors $\overline{AC}$ and $\overline{CB}$; that is, when A, B, C, D lie on a circle; and then

$$k^2 = \frac{BC}{AC}\,\frac{AD}{BD},$$

the anharmonic ratio of the four points A, D, C, B with respect to any other point on the circumscribing circle (Salmon, *Conic Sections*, Chap. IX).

Then we may put

$$\frac{z-a}{z-b}\frac{b-d}{a-d}=\operatorname{sn}^2(\xi+i\eta),$$

$$\frac{z-d}{z-b}\frac{a-b}{a-d}=\operatorname{cn}^2(\xi+i\eta),$$

$$\frac{z-c}{z-b}\frac{a-b}{a-c}=\operatorname{dn}^2(\xi+i\eta);$$

and therefore

$$\frac{r_1}{r_2}\frac{BD}{AD}=\operatorname{sn}(\xi+i\eta)\operatorname{sn}(\xi-i\eta),$$

$$\frac{r_4}{r_2}\frac{AB}{AD}=\operatorname{cn}(\xi+i\eta)\operatorname{cn}(\xi-i\eta),$$

$$\frac{r_3}{r_1}\frac{AB}{AC}=\operatorname{dn}(\xi+i\eta)\operatorname{dn}(\xi-i\eta).$$

Therefore, as before,

$$r_4AB-r_1BD\operatorname{dn}2\xi=r_2AD\operatorname{cn}2\xi,$$

$$r_4AB+r_1BD\operatorname{dn}2i\eta=r_2AD\operatorname{cn}2i\eta;$$

the vectorial equations of confocal orthogonal bicircular quartics, each of the form

$$l\rho+m\rho'+n\rho''=0;$$

with similar equations of the same curves, connecting r_1, r_2, r_3 and r_2, r_3, r_4.

Hence the theorems are proved (Salmon, *Higher Plane Curves*, Chap. VI.) that "*given four concyclic foci of a bicircular quartic, two such quartics can be described through any point, and these cut each other at right angles.*"

Also, if any cylinder, whose cross section is a bicircular quartic, be electrified, the electrification at any point will be inversely proportional to the square root of the product of the distances from the four real foci.

All these theorems are obtained immediately by the inversion of the system of confocal Cartesians of § 2, three foci of the inverse system of curves, the inverse points of the foci of the Cartesians lying on a circle passing through the centre of inversion, the fourth focus being at the centre of inversion; and then the vectorial equations are obtained by elementary geometry.

10. When two foci B, C of the three foci A, B, C of a system of confocal Cartesians coincide, then $k=1$, and we obtain the systems of confocal limaçons,

$$\left.\begin{aligned} r\cosh 2\xi - r' &= a \\ r\cos\ \ 2\eta + r' &= a \end{aligned}\right\},$$

where r is the distance of a point P from the double focus O, r' from the single focus A, and a is the distance between the foci.

Denoting the angle POA by θ, then

$$r'^2 = r^2 - 2ar\cos\theta + a^2,$$

and we obtain the polar equations of the limaçons

$$\left.\begin{aligned} r\sinh^2 2\xi &= 2a(\cosh 2\xi - \cos\theta) \\ r\sin^2 2\eta &= 2a(\cos\ \ \theta - \cos 2\eta) \end{aligned}\right\}.$$

If the foci B, C, after coincidence, now move at right angles to the original line of foci, so that ABC is an isosceles triangle, we obtain the system of curves derived from the integral

$$\int\frac{dz}{\sqrt{\{z-a\,.\,(z-m)^2+n^2\}}},$$

giving
$$z-a=\alpha\,\frac{1-\operatorname{cn}u}{1+\operatorname{cn}u},$$

where
$$\alpha=\sqrt{\{(m-a)^2+n^2\}}=AB \text{ or } AC,$$

and
$$k^2=\tfrac{1}{2}\left(1-\frac{m-a}{\alpha}\right)=\sin^2\tfrac{1}{4}BAC.$$

If r_1, r_2, r_3 denote the distances of a point from the three foci A, B, C, then

$$r_1^{\,2}=\alpha^2\,\frac{1-\operatorname{cn}(\xi+i\eta)}{1+\operatorname{cn}(\xi+i\eta)}\,\frac{1-\operatorname{cn}(\xi-i\eta)}{1+\operatorname{cn}(\xi-i\eta)}$$

$$=\alpha^2\left(\frac{\operatorname{cn}i\eta-\operatorname{cn}\xi}{\operatorname{cn}i\eta+\operatorname{cn}\xi}\right)^2$$

or
$$r_1=\alpha\,\frac{\operatorname{cn}i\eta-\operatorname{cn}\xi}{\operatorname{cn}i\eta+\operatorname{cn}\xi}.$$

Also
$$z-m-in$$

$$=a-m-in+\alpha\,\frac{1-\operatorname{cn}u}{1+\operatorname{cn}u}$$

$$=\alpha\left(\cos 2\theta-i\sin 2\theta+\frac{1-\operatorname{cn}u}{1+\operatorname{cn}u}\right)$$

(θ denoting the modular angle $\frac{1}{4}BAC$),

$$=2\alpha e^{-i\theta}\frac{k'-ik\operatorname{cn}u}{1+\operatorname{cn}u};$$

and therefore

$$r_2^{\,2}=4\alpha^2\frac{k'-ik\operatorname{cn}(\xi+i\eta)}{1+\operatorname{cn}(\xi+i\eta)}\;\frac{k'+ik\operatorname{cn}(\xi-i\eta)}{1+\operatorname{cn}(\xi-i\eta)}$$

$$=4\alpha^2\left(\frac{\operatorname{dn}\xi\operatorname{dn}i\eta+ikk'\operatorname{sn}\xi\operatorname{sn}i\eta}{\operatorname{cn}i\eta+\operatorname{cn}\xi}\right)^2,$$

or

$$r_2=2\alpha\frac{\operatorname{dn}\xi\operatorname{dn}i\eta+ikk'\operatorname{sn}\xi\operatorname{sn}i\eta}{\operatorname{cn}i\eta+\operatorname{cn}\xi};$$

and similarly

$$r_3=2\alpha\frac{\operatorname{dn}\xi\operatorname{dn}i\eta-ikk'\operatorname{sn}\xi\operatorname{sn}i\eta}{\operatorname{cn}i\eta+\operatorname{cn}\xi}.$$

Expressed in a real form

$$r_1=\alpha\;\frac{1-\operatorname{cn}\xi\operatorname{cn}\eta}{1+\operatorname{cn}\xi\operatorname{cn}\eta},$$

$$r_2=2\alpha\frac{\operatorname{dn}\xi\operatorname{dn}\eta-kk'\operatorname{sn}\xi\operatorname{sn}\eta}{1+\operatorname{cn}\xi\operatorname{cn}\eta},$$

$$r_3=2\alpha\frac{\operatorname{dn}\xi\operatorname{dn}\eta+kk'\operatorname{sn}\xi\operatorname{sn}\eta}{1+\operatorname{cn}\xi\operatorname{cn}\eta};$$

or

$$\frac{\alpha-r_1}{\alpha+r_1}=\operatorname{cn}\xi\operatorname{cn}\eta,$$

$$\frac{r_3-r_2}{r_3+r_2}=\operatorname{cn}(K-\xi)\operatorname{cn}(K'-\eta);$$

the elliptic functions of η being now to the complementary modulus k', and by the alternate elimination of ξ and η we obtain the vectorial equations of the orthogonal quartic curves, having foci at A, B, C.

11. Inverting the previous system of curves with respect to any point on the line through A perpendicular to BC, we obtain the system derived from the integral

$$\int\frac{dz}{\sqrt{\{z-a\,.\,z-c\,.\,(z-m)^2+n^2\}}}$$

having four foci A, B, C, D forming a kite-shaped figure; and then

$$\frac{z-a}{z-c}=\frac{\alpha}{\beta}\,\frac{1-\mathrm{cn}\,u}{1+\mathrm{cn}\,u},$$

where

$$\alpha=\sqrt{\{(m-a)^2+n^2\}}=AB \text{ or } AD,$$

$$\beta=\sqrt{\{(m-c)^2+n^2\}}=BC \text{ or } CD;$$

and

$$k^2=\tfrac{1}{2}\left(1+\frac{AB^2+BC^2-AC^2}{2AB.BC}\right)$$

$$=\tfrac{1}{2}(1+\cos ABC)$$

$$=\cos^2\tfrac{1}{2}ABC.$$

Then

$$\frac{r_1}{r_3}=\frac{\alpha}{\beta}\,\frac{\mathrm{cn}\,i\eta-\mathrm{cn}\,\xi}{\mathrm{cn}\,i\eta+\mathrm{cn}\,\xi}$$

and

$$\frac{r_3}{r_4}=\frac{\mathrm{dn}\,\xi\,\mathrm{dn}\,i\eta+ikk'\,\mathrm{sn}\,\xi\,\mathrm{sn}\,i\eta}{\mathrm{dn}\,\xi\,\mathrm{dn}\,i\eta-ikk'\,\mathrm{sn}\,\xi\,\mathrm{sn}\,i\eta},$$

and by the alternate elimination of ξ and η, we obtain the vectorial equations of the orthogonal quartic curves, having foci at A, B, C, D.

When $ABCD$ is a rhombus, the system of curves is as in § 5, given by

$$x+iy=\mathrm{cn}\,(\xi+i\eta).$$

Generally if we invert the system of curves of § 10 with respect to any point whatever, we obtain the second class of bicircular quartics, in which the four real foci do not lie on a circle, but are so related that

$$AB : BD :: AC : CD.$$

12. In order to solve the hydro-dynamical problems of determining the current and velocity functions of liquid motion due to the motion of cylinders whose cross-sections are curves $\xi=$ constant, or $\eta=$ constant; taking for example the confocal Cartesians defined by

$$x+iy=\mathrm{sn}^2(\xi+i\eta);$$

then since, expanded in a Fourier series,

$$\mathrm{sn}^2 u = \frac{K-E}{k^2K} - \frac{\pi^2}{k^2K^2}\Sigma n \frac{\cos n\pi \dfrac{u}{K}}{\cosh n\pi \dfrac{K'}{K}}.$$

therefore

$$x = \frac{K-E}{k^2K} - \frac{\pi^2}{k^2K^2}\Sigma n \frac{\cos n\pi \dfrac{\xi}{K} \cosh n\pi \dfrac{\eta}{K}}{\cosh n\pi \dfrac{K'}{K}}.$$

$$y = \qquad \frac{\pi^2}{k^2K^2}\Sigma n \frac{\sin n\pi \dfrac{\xi}{K} \sinh n\pi \dfrac{\eta}{K}}{\cosh n\pi \dfrac{K'}{K}}.$$

and therefore if

$$\psi = \frac{\pi^2 U}{k^2K^2}\Sigma n \frac{\sin n\pi \dfrac{\xi}{K} \sinh n\pi \dfrac{\alpha}{K} \sinh n\pi \dfrac{\eta-\beta}{K}}{\cosh n\pi \dfrac{K'}{K} \sinh n\pi \dfrac{\alpha-\beta}{K}},$$

then $\psi = 0$ when $\eta = \beta$,

$\psi = Uy$ when $\eta = \alpha$,

and $$\frac{d^2\psi}{d\xi^2} + \frac{d^2\psi}{d\eta^2} = 0;$$

and therefore ψ is the current function for the motion of the liquid between the cylinder, $\eta = \alpha$ and $\eta = \beta$, when the cylinder $\eta = \alpha$ is moving parallel to the axis of x with velocity U and the cylinder $\eta = \beta$ is fixed.

If ϕ denote the velocity function of this motion, being the conjugate function to ψ,

$$\phi = \frac{\pi^2 U}{k^2K^2}\Sigma n \frac{\cos n\pi \dfrac{\xi}{K} \sinh n\pi \dfrac{\alpha}{K} \cosh n\pi \dfrac{\eta-\beta}{K}}{\cosh n\pi \dfrac{K'}{K} \sinh n\pi \dfrac{\alpha-\beta}{K}}.$$

In a similar manner the current and velocity functions due to any motion of translation or rotation of any of the cylinders, $\xi =$ constant, or $\eta =$ constant, may be written down.

(2) *On a Mathematical Law of Interest in Political Economy.* By Dr Akin-Karoly.

May 9, 1881.

PROFESSOR NEWTON, PRESIDENT, IN THE CHAIR.

E. J. Sing, B.A., Christ's College, and R. I. Lynch, Curator of the Botanic Garden, were balloted for and duly elected associates of the Society.

The following communications were made to the Society:

(1) *On the probable secular change in the position and aspect of the Constellation Ursa Major.* By J. B. PEARSON, D.D.

In the Homeric Poems, which without prejudice may fairly be fixed about 750 B.C., this constellation is spoken of as distinctly circumpolar:

οἴη δ' ἄμμορός ἐστι λοετρῶν Ὠκεανοῖο.
Iliad, Σ. 489, *Odyss.* E. 275.

Now the latitude of Alexandria, practically the most southern point that could have been visited at that era by Greek navigators, being about 31° 10′: any star, to be circumpolar there, must be in about 58½° north declination. At the present time, only one of the seven chief stars of the constellation Ursa Major is so, being in N. Dec. 60° 26′: the rest being less, and one so low as 49° 57′. If however we employ the method of computation indicated in a former paper (*Proc.* Vol. III. p. 70, &c.), viz. find the present Latitude and Longitude of each separate star; subtract from the Longitude an amount proportional to the annual precession of 50″·1 continued during a period of about 2600 years, and then find the R. A. and Dec. of each star at the commencement of the period, we shall find that the star η, the one with the least North Dec., was then in N. Dec. 64°, β and γ between 67° and 68°, δ, ϵ, ζ 70° to 71° and α nearly 73°. Clearly in those days the constellation fulfilled the conditions of the Homeric verse in a very different way to what it does now. I may as well mention that the two pointers are α and β, and η the star at the other extremity of the constellation.

The annexed Table indicates the present positions of the several stars, and also what they would have been in 750 B.C. and A.D. 1; the upper line obviously answering to the first, and the lower to the second date:

	A.D. 1872.		B.C. 750 and A.D. 1.		
	R. A.	N. Dec.	R. A.	Error of Latitude.	N. Dec.
	H. M. SEC.	° ′ ″	H. M. SEC.		° ′ ″
α	10 · 55 · 48½	62 · 26 · 30	7 · 0 · 56 8 · 24 · 35	38½′	72 · 46 · 34 70 · 54 · 20 ...(α)
β	10 · 54 · 6	57 · 4 · 5	7 · 24 · 30 8 · 36 · 12	35′	67 · 46 · 28 65 · 41 · 0 ...(β)
γ	11 · 47 · 5	54 · 24 · 24	8 · 47 · 48 9 · 49 · 50	36′	67 · 24 · 0 64 · 14 · 0 ...(γ)
δ	12 · 9 · 5	57 · 44 · 38	9 · 14 · 20½ 10 · 17 · 15	41′	71 · 21 · 37 67 · 54 · 0 ...(δ)
ε	12 · 48 · 23½	56 · 39 · 19	10 · 21 · 10½ 11 · 12 · 52	37′	70 · 55 · 10 66 · 58 · 47 ...(ε)
ζ	13 · 18 · 46½	55 · 35 · 35	11 · 11 · 46 11 · 54 · 33	41′	69 · 53 · 45 65 · 46 . 47 ...(ζ)
η	13 · 42 · 29½	49 · 57 · 11	11 · 44 · 51 12 · 12 · 14	23′	64 · 2 · 22 58 · 24 · 1 ...(η)

N.B. The fifth column shews the difference between the latitude of each star according to my own computation and that given in Ptolemy's catalogue, as printed by Mr Baily, in the *Memoirs of the Royal Astronomical Society*, Vol. XIII. My own latitudes are always greater than Ptolemy's.

About the date of the construction of Ptolemy's Catalogue of Stars in its present form, fixed by Bode, the German astronomer, at 63 A.D. (Knobel); or rather at the date A.D. 1 to which I have reduced my own calculations, the star η was about circumpolar at Alexandria, being in North Dec. 58° 24′. The next lowest however, γ, was in 64° 14′, and so easily circumpolar. Since that date

the various stars of the constellation have been gradually increasing their distance from the true or virtual Pole; which, *vice versa*, it may be observed has been gradually approaching the star we call the Pole-Star: the Declination of which must have been about 78° 20′ in Ptolemy's time, and 74° 20′ seven centuries earlier, its R.A. having during the same period advanced from 22 h. 53 m. at the earliest of these two eras, and 23 h. 15 m. at the next, to 1 h. 15 m. its present approximate amount.

It will be understood that only comparative accuracy is claimed for the results given in this paper. The latitudes of the stars discussed which result from the computations are on an average about 40′ greater than those given in the catalogue ascribed to Ptolemy: and it is a somewhat imperious solution of the difficulty to say that Ptolemy was wrong. There are also two statements of Ptolemy's, cited by Delambre, which seem to imply the possibility of secular changes in other points which cannot be directly inferred from existing phenomena. The present rate of change in the obliquity of the ecliptic is given by the best authorities as about 48″ a century: which would make the actual obliquity 23° 42′ in Ptolemy's time: whereas the value given by him is 23° 51′. In his Geography again we find the latitude of Alexandria and Syene both given about 15′ too small. It is true that the latitudes of the various places given by him in this work, and scattered all over Europe, are so erroneous that no general inference can be safely drawn from them; but the latitude of Alexandria was one of the points fixed even by Eratosthenes much before Ptolemy's time; so was that of Syene in Upper Egypt; and if we add Rome as a place the latitude of which was likely to have been accurately ascertained we have thus three places of note, the errors about which are almost identical. We have in fact

	Latitude according to Ptolemy.	True latitude.	Error.
Alexandria	30°·58′	31°·12′	14′·(–)
Syene (Assouan)	23°·50′	24°· 5′	15′·(–)
Rome	41°·40′	41°·54′	14′·(–)

a table which certainly shews a very nearly equal error in each case.

It is true the line of argument I am following is not very strict, but those who consider the immense pains bestowed on practical astronomy in the Alexandrian era will not I think be satisfied without some effort to see how far the results then thought to be ascertained harmonize with those of modern science.

(2) *On Sympathetic Needles.* By J. B. Pearson, D.D.

I propose in this paper to give a historical sketch, as well as I can trace it out, of the notion prevailing some hundred years ago, as to the possibility of magnetic signalling between places at some distance from one another. After a description of the idea itself, I will state how and when it seems to have arisen—and passed into obscurity.

The notion itself is perhaps most neatly explained by Addison, in the *Spectator*, No. 241 (Dec. 6, 1711).

"Strada, in one of his Prolusions, gives an account of a chimerical correspondence between two friends by the help of a certain Loadstone, which had such virtue in it, that if it touched two several needles, when one of the needles so touched began to move, the other, though at never so great a distance, moved at the same time and in the same manner. He tells us that the two friends, being each of them possessed of one of these needles, made a kind of dial-plate, inscribing it with the four and twenty letters, in the same manner as the hours of the day are marked upon the ordinary dial-plate. They then fixed one of the needles on each of these plates in such a manner, that it could move round without impediment, so as to touch any of the four and twenty letters. Upon their separating from one another into distant countries, they agreed to withdraw punctually into their closets at a certain hour of the day, and to converse with one another by means of this their invention. Accordingly when they were some hundred miles asunder, each of them shut himself up in his closet at the time appointed, and immediately cast his eye upon his dial-plate. If he had a mind to write anything to his friend, he directed his needle to every letter that formed the words that he had occasion for, making a little pause at the end of every word or sentence, to avoid confusion. The friend in the meanwhile saw his own sympathetic needle moving of itself to every letter which that of his correspondent pointed at. By this means they talked together across a whole continent, and conveyed their thoughts to one another in an instant over cities or mountains, seas or deserts.

"If Mons. Scudery or any other writer of romance had introduced a necromancer, who is generally in the train of a knight errant, making a present to two lovers of a couple of these above mentioned needles, the reader would not have been a little pleased to have seen them corresponding with one another, when they were guarded by spies and watches, or separated by castles and adventures. In the meantime, if ever this invention should be revived or put in practice, I would propose that on the lover's dial-plate there should be written, not only the four and twenty letters, but several entire words which have always a place in

passionate epistles, such as Flames, Darts, Die, Languish, Absence, Cupid, Heart, Eyes, Hang, Drown, and the like. This would very much abridge the lover's pains in this way of writing a letter, as it would enable him to express the most useful and significant words with a single touch of the needle."

I have transcribed the passage from Addison at full length, in order that the spirit in which he wrote may be properly appreciated. I give next the passage in the *Prolusiones Academicæ* of Fam. Strada, in which it seems to be allowed that the notion in general is first promulgated. Strada was a Jesuit, who lived mainly at Rome, about the year 1600: he also wrote an history of the wars in the Low Countries in the sixteenth century which is well spoken of by Mr Motley; but the work in which the following passage is found is a book written in Latin, on the model of Cicero's philosophical works, evidently designed to exercise students in language, style, and general knowledge. The passage referred to by Addison is as follows:

P. Bembus loquitur:

Magnesi genus est lapidis mirabile, cui si
Corpora ferri plura stylosve admoveris, inde
Non modo vim motumque trahent, quo semper ad Ursam
Quæ lucet vicina polo se vertere tentent,
Verum etiam mirâ inter se ratione modoque;
Quotquot enim lapidem tetigere styli, simul omnes
Conspirare situm motumque videbis in unum.
Ut si fortè ex his aliquis Romæ moveatur,
Alter ad hunc motum, quamvis sit dissitu' longè,
Arcano se naturai fœdere vertat.
Ergò age, si quid scire voles qui distat, amicum,
Ad quem nulla accedere possit epistola, sume
Planum orbem patulumque: notas elementaque prima
Ordine quo discunt pueri, describe per oras
Extremas orbis: medioque repone jacentem,
Qui tetigit magneta, stylum; ut versatilis inde
Litterulam quamcunque velis, contingere possit.
Hujus ad exemplum, simili fabricaveris orbem
Margine descriptum, munitumque indice ferri,
Ferri quod motum magnete accepit ab illo.
Hunc orbem discessurus sibi portet amicus,
Conveniatque priùs, quo tempore queisve diebus
Exploret stylus an trepidet, quidve indice signet.
His ita compositis, si clam cupis alloqui amicum,
Quem procul a tete terrai distinet ora,
Orbi adjunge manum, ferrum versatile tracta.
Hic disposta vides elementa in margine toto:
Queis opus est ad verba notes, huc dirige ferrum.

Litterulasque, modo hanc modo et illam, cuspide tange,
Dum ferrum per eas iterumque iterumque rotando
Componas singillatim sensa omnia mentis.
Mira fides. Longe qui distat cernit amicus
Nullius impulsu trepidare volatile ferrum,
Nunc huc nunc illuc discurrere; conscius hæret,
Observatque styli ductum sequiturque legendo
Hinc atque hinc elementa, quibus in verba coactis
Quid sit opus sentit, ferroque interprete discit.
Quin etiam cum stare stylum videt, ipse vicissim
Si quæ respondenda putat, simili ratione
Litterulis variè tactis, rescribit amico.
O! utinam hæc ratio scribendi prodeat usu.
Cautior et citior properaret epistola, nullas
Latronum verita insidias, fluviosque morantes.
Ipse suis Princeps manibus sibi conficeret rem;
Nos soboles scribarum, emersi ex æquore nigro,
Consecraremus calamum Magnetis ad oras.

STRADA, p. 306, (Ed. Colon. 1617).

It will be allowed that Addison very fairly represents Strada's meaning. I would only observe that Strada does not say that the thing ever had been done, but that if the needles are magnetized the result follows with those who use them in the proper way: the difference it is true is comparatively slight.

I will now give an account of the various notices which we find of the idea in successive writers down to the middle of the eighteenth century. Since then, I think philosophy or common sense would have precluded its reappearance, until it was either re-invented or else utilized, in the construction of the electric telegraph. Possibly some one conversant with the early history of this instrument may be able to inform the public whether there is any reason to think that our first telegraphists in the method they adopted, were aware they were employing one which in a certain degree is, theoretically speaking, so ancient.

Strada's book was evidently well known, and probably much used in education, during the first half of the seventeenth century. In the library of my own college there are three copies, printed in 1617, '19, '25; and I find that there are as many in the University Library, and also in that of St John's College, and probably it may easily be found elsewhere. It was also reprinted at Oxford in 1662; but I cannot find a copy of this edition in Cambridge. Consequently the book was well known at the time, and as a consequence, we find several references to it in writers of the period. The first two references I owe to *Notes and Queries*. In that periodical, 1st Ser. Vol. XI. 459, a correspondent refers to the work *Recreations Mathematiques*, by Denis Henrion, a French mathe-

matician of the first half of the seventeenth century, who also wrote on the compass, and on the micrometer, or an instrument which he calls by that name. He is said to describe Strada's imaginary dials pretty literally, only giving names, Jean and Claude, to the supposed friends: he does not seem however to have been at all taken in, as he adds, "It is a fine invention, but I do not think there is a magnet in the world which has such a virtue; besides it is inexpedient, for treasons would be too frequent and too much protected." Henrion's article is said to be illustrated with a dial inscribed with the letters of the Alphabet and furnished with a needle as an index, the needle turning on a pivot in the centre: but the book is not in the Cambridge Library, or the Bodleian Catalogue. Though the correspondent of *Notes and Queries* cites from an edition of 1662, it appears from the *Biographie Universelle* that the book was published as soon as 1627: and this explains who is the author who is referred to by a German, Schwentner, in a work called *Deliciæ Physico-mathematicæ,* published in 1636, and referred to in *Notes and Queries,* 2nd Ser. Vol. IV. 461: where the same description recurs, the names being given as Claudius and Johannes, which virtually proves that he borrowed from Henrion.

Sir T. Browne, in his *Vulgar Errors,* Bk. II. c. 3, 1649, again brings up the notion, and says that he had experimented on it by "framing two circles of wood, on which he placed two stiles or needles composed of the same steel, touched with the same loadstone, and at the same point." He candidly adds, as may be expected, that no motion of any kind followed; he admits however that with a somewhat similar experiment he had a qualified success, the description of which I give, because I am not sure that I quite understand his method, and because those who are in the habit of using magnets may like to repeat it. He says, after referring to certain authors who "deliver many ways to communicate thoughts at a distance;"..."this we will not deny may in some manner be effected by the loadstone; that is, from one room into another; by placing a table in the wall common to both, and writing thereon the same letters one against another: for upon the approach of a vigorous loadstone unto a letter on this side, the needle will move unto the same on the other. But this is a very different way from ours at present: and hereof there are many ways delivered, and more may be discovered which contradict not the rule of its operations." Not long after Browne, Glanvill, in his *Vanity of Dogmatizing* (1661), p. 204, refers again to the idea, and criticizes the sufficiency of the test applied by Browne on grounds that, as far as I can see, only prove that he had never read carefully what Browne says: whose general meaning to me is clear and rational enough. Glanvill is well known in the history of that time as an original thinker, and as one of the earliest pro-

moters of the Royal Society: weak as his philosophical instinct undoubtedly is on this special point, he proves that the theory of these needles must have been familiar to the educated men of that period. His opinion is cited at length in *Notes and Queries*, 2nd Ser. Vol. IV. 392: Browne's experiment and opinion being given at p. 266 of the same volume.

Half a century later, when Addison introduces the subject in the *Spectator*, it will be noticed that he refers to Strada's writings in a way which evidently implies that they were known to his contemporaries. We may however also suppose that the word *chimerical* which he uses fairly represents the general opinion on the subject: though I have some doubts whether Johnson, when he wrote his life of Browne (1756), could have known of the passage in the *Spectator*, or Strada's writings, from the way he speaks of him as owing his knowledge of the theory about the needles to a "flying rumour."

A very fair English metrical version of Strada's lines is given in *Notes and Queries*, 1st Ser. Vol. VI 204, taken from a publication called *The Student: or the Oxford and Cambridge Miscellany*, published in 1750. The passage from Addison had been given at p. 93 of the same volume: published in 1852. As the electric telegraph at that time had just come into general use, we may be glad to see that our *literati* were not behind-hand in looking up the history, if it may be called so, of the subject.

I may mention that in a magazine, called the *Scots Magazine*, of the date of 1753, *N. & Q.* 1st Ser. IX. 274 (*not* 1653, as erroneously given, *N. & Q.* Vol. VIII. 364), we find the opinion that a method of communication by electricity of a somewhat similar kind was practicable: but there is no direct reference to Strada or those who avowedly borrowed from him.

We can see from the authorities already cited that the notion of "sympathetic needles" as they may be generally called, cannot be traced farther than Famianus Strada, a literary character, who was born in 1572: was admitted as a Jesuit at Rome, where he generally lived; and died in 1649. If any one will look through the list of his writings, it will be seen at once that there is no reason to think that the verses I have printed were anything else than a literary exercise; and though he was probably a historian of merit, there is nothing at all to indicate that he was in any way given to natural philosophy of any kind. As however he fictitiously assigns the description of his magnetic needles to so well known a personage as Pietro Bembo, the secretary of Pope Leo X., it seems not unreasonable to think that there may have been a tradition at Rome that Bembo practised or was acquainted with some device of the kind. His pursuits unquestionably were literary rather than scientific: but he was a collector of coins and

other objects of ancient art as well as of manuscripts[1]: and one of his sons was the possessor of the *Tabula Isiaca*, a celebrated antique, now generally thought to be of the age of Hadrian, whether genuine Egyptian work or no; and which is now preserved at Turin. Had Bembo some magnetical contrivance for signalling which cannot now be exactly known? and some information respecting which had come into the hands of Strada in the following century. I cannot suggest to myself a better explanation of Strada's verses.

P.S. As I have so frequently referred to *Notes and Queries*, I wish to say how far I am indebted to that valuable periodical for the references which I have given. My attention was first drawn to the subject by Glanvill, in the course of my ordinary reading: I was thus led to refer to the *History of the Electric Telegraph*, by Prescott (*Boston*, U.S. 1860), who ascribes the first idea of the kind to Strada; and to Moigno's work on the subject, where I found references to Strada, and also to Addison. I then thought of *Notes and Queries* as a useful source of general information. The reference to Browne, I think I got from *N. & Q.*, or perhaps from Glanvill. My object in adding this is to avoid the imputation of plagiarizing.

A SPECIAL GENERAL MEETING of the Society which had been called by the Council in accordance with Section VIII. of the Bye-Laws, was then held for the consideration of the following Report of the Council which had been circulated among the Fellows of the Society.

REPORT OF THE COUNCIL OF THE CAMBRIDGE PHILOSOPHICAL SOCIETY.

IT will be remembered that at the last Annual General Meeting the President made the following remarks respecting the Society's Library:

"The next point upon which I will touch is one that is likely to be of great importance to the Society. Fellows are aware that during the past Vacation, some long-projected alterations of the building in which we are assembled have been completed, with the result of throwing into one two smaller rooms. The large chamber thus formed is at present unoccupied, but it is no secret (for the assertion was made openly in the Schools) that the intention of some of those who brought about this change was to provide a library for the accommodation of scientific books for the use of all

[1] Roscoe, *Life of Leo X.* III. 195.

those who use this building. The fact that such a library would sooner or later become necessary has been long foreseen, and, for myself, the time of need seems now to have arrived. This proposal has many times been discussed in private, and I believe it has been openly urged that the library of the Society should form the nucleus of the new collection. A week ago the subject was brought formally before the Council of the Society, and a Committee appointed to report thereon. It will be plain to all that the Society would be a great gainer if its books could be accommodated in a more accessible room than that which they now occupy, and members would without doubt find the large chamber on the ground floor very commodious for their purposes. But, on the other hand, there are necessarily some disadvantages, supposing that this plan was carried out. In the first place, the books thus being so much more accessible would have to be put under a much stricter supervision than at present, and this supervision could not be attained without some expense. Then, too, it has been announced that this large room will require to be used for examinations, and it is of course obvious that in that case unrestricted admission could not be at all times enjoyed even by Fellows of the Society; while we all know, from the experience of past years, the tendency of examinations to increase both in number and in duration. Fellows of the Society might therefore come to find that the projected change would involve a serious deprivation of their rights. Yet, with all this, I fully believe that the Society would on the whole benefit by the alteration, and I trust that means may be found whereby the inconvenience I have mentioned may be reduced to a minimum. More than this it is impossible for me to say at present, and of course it will be understood that the Society will have due notice given to it of any action taken by the Council, and an opportunity of expressing its opinion thereon, but I have thought it only right to take the present opportunity of announcing to the Society at this meeting what is in contemplation, and the possibility of some arrangement being entered into with the University by the Council subject to the Society's approval."

The Committee spoken of above having reported to the Council, it was decided that the following letter be addressed by the President to the Vice-Chancellor:

25 October, 1880.

Dear Mr Vice-Chancellor,

The Cambridge Philosophical Society occupies, as you are aware, a room in the New Museums which is used for the meetings of the Council of our Society, and also as a Library. The books have now become so numerous that a larger apartment is required for their convenient accommodation.

The want of a central Scientific Library in the New Museums, for the use of the Professors, Lecturers, and students, has long been felt, and recognized in various ways by the University.

If such a Library were founded and placed in a suitable room, the Council of the Philosophical Society would be prepared to recommend to the Society that their Library should be deposited in it, under regulations to be approved by the Society. It would thus form a nucleus for such a collection of books as is required, which, there is reason to believe, would before long be largely increased by donations.

I remain, dear Mr Vice-Chancellor,

ALFRED NEWTON

(*President of the Cambridge Philosophical Society*).

At the meeting of the Council, held 8 November, the following Minutes were made:

'A letter was read from the Vice-Chancellor, acknowledging the receipt of the President's letter, and stating that he had forwarded it to the Museums and Lecture Rooms Syndicate.

'A letter was also read from Mr J. W. Clark, the Secretary of the Museums and Lecture Rooms Syndicate, containing the following Minute of the Syndicate: "A Sub-Syndicate was appointed to confer with the Philosophical Society, and to prepare a draft report on the subject for the use of the Syndicate."'

A Committee was then appointed to confer with the Sub-Syndicate above mentioned, and to report to the Council. They reported as follows:

'At a Conference of the Members of the Sub-Syndicate appointed by the Museums and Lecture Rooms Syndicate, and of the Members of the Committee appointed by the Council of the Cambridge Philosophical Society, held 10 December, 1880, it was agreed that:

'1. The Library of the Philosophical Society, consisting of about 5000 volumes, has become too large to be conveniently accommodated in the room in the New Museums where it is at present placed.

'2. This Library, consisting chiefly of scientific periodicals and the publications of learned Societies (British and foreign), which last are received by the Society in exchange for its own publications, would, if rendered more accessible to the Professors, officers and students engaged at the New Museums and Lecture Rooms, be a most valuable assistance to them in the execution of their duties and prosecution of their studies.

'3. The recent alteration in the central block of buildings of the New Museums has given a large room at present unappropriated, which from its size and convenient situation seems admirably fitted, among other purposes, for a library.

'4. If the books of the Philosophical Society could be removed to this large room, and be placed under proper custody and regulations so that they could be used by those who work in the New Museums and Lecture Rooms, the advantages above indicated (2) would, it is believed, be attained.

'5. In this case the services of a fit librarian would be required for the proper custody of the books and the keeping of the catalogue, but the funds of the Philosophical Society are not, nor are they likely to be, sufficient to pay the salary of such an officer; all its income being required for the expenses attending its own publications in exchange for which so many valuable works are received, subscription to scientific periodicals, and purchase of other additions to the Library.

'6. It would therefore be necessary that the librarian should be paid by the University, and presuming that his services would be required for six hours daily during full Term and certain portions of the Vacations—especially in the months of July and August—his salary might be estimated at not less than £70 *per annum*, but it is possible that this sum might be somewhat reduced, if any arrangement could be made with the University Library Syndicate for partly employing one of the Library-Assistants, and thus ensuring the services of one properly trained in library work.

'7. The management of the Philosophical Society's Library might be entrusted to a Committee of (say) six persons, one half to be appointed by the Museums and Lecture Rooms Syndicate, and the other half by the Council of the Society, but it would be expedient that a few definite conditions should at the outset be agreed upon by the University and the Society, which should not be altered except with the express leave of both.

'8. These conditions should among other things define the position of the Fellows of the Philosophical Society as regards the privilege of removing books from the Library, especially in view of access to the room being forbidden when required for examinations; and should also pledge the Society as a body to such outlay in the purchase of new works and the continuance of its subscription to scientific periodicals as its income will admit—due allowance being made for its other necessary expenses.

'9. It seems inexpedient at present to enter into further details—but if the considerations urged above find favour with the University and the Society—it is hoped that the details required

to give effect to them may without much difficulty be arranged in a manner satisfactory to both parties.'

This Report was adopted by the Council, and the Committee was requested to continue its services, and to make suggestions with regard to the proposed arrangement. After further preliminary negotiations between the Council and the Syndicate, a draft of the Conditions referred to in § 7 and § 8 of the Report so far as they relate to the privilege of removing books from the Library, was adopted in the following form by the Council of the Philosophical Society, and the Syndicate have since expressed their general approval of them and of the whole scheme.

'I. That no book be taken out of the buildings of the New Museums, except by Fellows of the Cambridge Philosophical Society; and by them only for a limited time, not exceeding in any case seven days.

'II. That every book removed from the Library be entered in a register to be kept for the purpose.

'III. That in addition to the Fellows of the Society all Professors, Demonstrators, Teachers recognized by the University, and Curators, together with such persons as the Committee of Management may from time to time decide, be permitted to use the books of the Library within the buildings of the New Museums.

'IV. That certain books be marked "*Not to be taken out;*" provided that books so marked may be taken out by Fellows of the Society during the last hour in any day on which the Library is open; and further, provided that they be brought back during the first two hours that the Library is next open.

'V. That it be the duty of the Committee of Management specified in the Report already referred to, § 7, to decide from time to time which books shall be marked "*Not to be taken out;*" and to enforce the payment of fines imposed for the breach of any of these rules.

'VI. That subject to conditions necessary for the safety of the Library the Fellows of the Philosophical Society have access, so far as is practicable, to the Library, at hours other than those during which it is generally open.

'VII. That these rules shall apply of necessity only to the books which are or shall become the property of the Philosophical Society.'

The Council think that the time is now come to lay before the Society a statement of these preliminary negotiations. The Council recommend:

I. That they be authorized by the Society to offer the use of

the Library to the University on the following conditions, and to take the necessary steps for that purpose.

1. That the books be deposited in the large room on the ground floor of the central block of the New Museums.

2. That the University undertake to make provision for the necessary fittings in the New Library, and for the services of a fit Librarian for the proper custody of the books and the keeping of a catalogue.

3. That the Society undertake to expend yearly, as heretofore, such sums in the purchase of new works and subscriptions to scientific periodicals, and in binding, as its income will admit—due allowance being made for its other necessary expenses.

4. That the management of the Library be entrusted to a Committee of six persons, one half to be appointed by the Museums and Lecture Rooms Syndicate and one half by the Council of the Philosophical Society.

Provided that the fundamental regulations for the Library, marked I.—VII. in the foregoing Report, be not altered except with the consent both of the Society and of the University.

II. That on the acceptance by the University of the foregoing conditions and on the removal of the Society's Library into the new room, Chapter XIII. of the Bye Laws of the Society be repealed.

Signed on behalf of the Council,

ALFRED NEWTON, *President.*

April 25, 1881.

It was moved by Mr E. Hill and seconded by the Master of Gonville and Caius College, that this Report be confirmed. The motion was carried unanimously.

May 23, 1881.

PROFESSOR NEWTON, PRESIDENT, IN THE CHAIR.

The following communications were made to the Society:

(1) *On the elliptic-function solution of the equation* $x^3+y^3-1=0$. By Professor CAYLEY.

I had occasion to find elliptic-function expressions for the coordinates (x, y) of a point on the cubic curve $x^3+y^3=1$. These

are derivable from the formulæ given, Legendre, *Fonctions Elliptiques*, t. I. pp. 185, 186, for the reduction to elliptic integrals of the integral $R = \int \frac{dr}{(1-z^3)^{\frac{2}{3}}}$, viz. Legendre writing

$$z = \frac{\sqrt{4y^3-1} - \sqrt{3}}{\sqrt{4y^3-1} + \sqrt{3}},$$

and then $\qquad m^3 = 2$ and $m^2 y = 1 + x^2$,

finds first

$$R = m\sqrt{3} \int \frac{dx}{\sqrt{x^4 + 3x^2 + 3}};$$

and then writing $r = \sqrt[4]{3}$, $x = \tan \frac{1}{2}\phi$, and $c^2 = \frac{1}{4}(2 - r^2)$, finds

$$R = \tfrac{1}{2} mr \int \frac{d\phi}{\sqrt{1 - c^2 \sin^2 \phi}};$$

we have therefore only to write $\sin \phi = \operatorname{sn} u$, to modulus

$$c, = \tfrac{1}{2}\sqrt{2 - \sqrt{3}},$$

and we thence obtain an expression for z in terms of the elliptic functions $\operatorname{sn} u$, $\operatorname{cn} u$, $\operatorname{dn} u$.

Writing x instead of z, and k for c, then

$$m = \sqrt[3]{2}, \quad r = \sqrt[4]{3}; \quad k = \tfrac{1}{2}\sqrt{2 - r^2}, \quad k' = \tfrac{1}{2}\sqrt{2 + r^2}.$$

And working out the substitutions, the resulting formulæ are

$$x = \frac{2r \operatorname{sn} u \operatorname{dn} u - (1 + \operatorname{cn} u)^2}{2r \operatorname{sn} u \operatorname{dn} u + (1 + \operatorname{cn} u)^2},$$

$$y = \frac{m(1 + \operatorname{cn} u)\{1 + r^2 + (1 + r^2)\operatorname{cn} u\}}{2r \operatorname{sn} u \operatorname{dn} u + (1 + \operatorname{cn} u)^2},$$

where the modulus is k as above; and these values give

$$x^3 + y^3 = 1,$$

$$\frac{dx}{(1-x^3)^{\frac{2}{3}}}, = \frac{-dy}{(1-y^3)^{\frac{2}{3}}} = \tfrac{1}{2} mr\,du.$$

The verification is interesting enough; starting from the expression for x, and for shortness representing it by

$$x = \frac{A - B}{A + B},$$

we have

$$1-x^3=\frac{2B(3A^2+B^2)}{(A+B)^3}, =\frac{m^3(1+\text{cn}\,u)^2(3A^2+B^2)}{\{2r\,\text{sn}\,u\,\text{dn}\,u+(1+\text{cn}\,u)^2\}^3}.$$

We find

$$3A^2+B^2=12r^2\,\text{cn}^2 u\,\text{dn}^2 u+(1+\text{cn}\,u)^4,$$

$$=(1+\text{cn}\,u)\{12r^2(1-\text{cn}\,u)(k'^2+k^2\,\text{cn}^2\,u)+(1+\text{cn}\,u)^3\},$$

where the term in { } is in fact a perfect cube

$$=[1+\text{cn}\,u+r^2(1-\text{cn}\,u)]^3.$$

(The last mentioned expression is in fact

$$=(1+\text{cn}\,u)^3+r^2(1-\text{cn}\,u)\,[3(1+\text{cn}\,u)^2+3r^2(1+\text{cn}\,u)(1-\text{cn}\,u)$$
$$+r^4(1-\text{cn}\,u)^2],$$

where the second term is

$$=12r^2(1-\text{cn}\,u)\,[\tfrac{1}{2}(1+\text{cn}^2 u)+\tfrac{1}{4}r^2(1-\text{cn}^2 u)],$$

that is, it is

$$=12r^2(1-\text{cn}\,u)(k'^2+k^2\,\text{cn}^2\,u)):$$

we have consequently

$$1-x^3=\frac{m^3(1+\text{cn}\,u)^3\{1+r^2+(1-r^2)\,\text{cn}\,u\}^3}{\{2r\,\text{sn}\,u\,\text{dn}\,u+(1+\text{cn}\,u)^2\}^3},$$

or extracting the cube root $y, =\sqrt[3]{1-x^3}$, has its foregoing value: and the differential expressions are then verified.

Suppose $y=1$, we have

$$(m-1)(1+\text{cn}\,u)^2+mr^2(1-\text{cn}^2\,u)=2r\,\text{sn}\,u\,\text{dn}\,u,$$

that is

$$(m-1)^2(1+\text{cn}\,u)^3+2m(m-1)r^2(1+\text{cn}\,u)^2(1-\text{cn}\,u)$$
$$+3m^2(1+\text{cn}\,u)(1-\text{cn}\,u)^2=r^2(1-\text{cn}\,u)\{4-4k^2(1-\text{cn}^2\,u)\}$$

or observing that the right-hand side is

$$=r^2(1-\text{cn}\,u)\{(1+\text{cn}\,u)^2+(1-\text{cn}\,u)^2+r^2(1+\text{cn}\,u)(1-\text{cn}\,u)\}$$

and multiplying by $\frac{1}{3}r^2$, the equation becomes

$$0=\tfrac{1}{3}(m-1)^2r^2(1+\text{cn}\,u)^3+(2m^2-2m+1)(1+\text{cn}\,u)^2(1-\text{cn}\,u)$$
$$+(m^2-1)r^2(1+\text{cn}\,u)(1-\text{cn}\,u)^2-(1-\text{cn}\,u)^3;$$

viz. this is

$$0=\{\tfrac{1}{3}r^2(m^2-1)(1+\text{cn}\,u)-(1-\text{cn}\,u)\}^3,$$

as is immediately verified: hence writing $\frac{1}{3}r^2=\frac{1}{r^2}$, we have for the value in question, $y=1$,

$$(m^2-1)(1+\text{cn}\,u)-r^2(1-\text{cn}\,u)=0,$$

or say

$$m^2(1+\text{cn}\,u)=(1+\text{cn}\,u)+r^2(1-\text{cn}\,u),$$

that is

$$\text{cn}\,u=\frac{r^2+1-m^2}{r^2-1+m^2},$$

which is one of the values of cn u derived from the equation $x=0$; but this equation $x=0$ gives, not the foregoing equation, but

$$m^6(1+\text{cn}\,u)^3=\{(1+\text{cn}\,u)+r^2(1-\text{cn}\,u)\}^3,$$

viz. the three values of cn u are the foregoing value and the two values obtained therefrom by changing m into ωm and $\omega^2 m$ respectively, ω being an imaginary cube root of unity. In fact the curve $x^3+y^3=1$, has at the point $x=0$, $y=1$ an inflexion, the tangent being $y=1$, so that this line meets the curve in the point counting three times; but the line $x=0$ meets the curve in the point, and besides in two imaginary points.

(2) *Continued observations on the state of an eye affected with a peculiar malformation.* By Sir GEORGE BIDDELL AIRY, K.C.B., M.A., LL.D., D.C.L., Honorary Fellow of Trinity College, Astronomer Royal.

Nearly ten years have elapsed since I last reported to the Society the state of my eyes, as regards optical convergence of pencils of rays. I subjoin the results of an examination lately made, and I place them in series with those of preceding examinations, as serving to shew clearly the gradual change which takes place in the eye during a period exceeding 55 years.

I. Distance from the cornea of the left eye at which a luminous point presents the appearance of a nearly horizontal line.

In 1825, 3·5 inches;	Reciprocal = ·286;		
		Difference =	– ·073.
In 1846, 4·7 ...... ;	.... ·213;		
		....	– ·028.
In 1866, 5·4 ...... ;	.... ·185;		
		....	– ·006.
In 1871, 5·6 ...... ;	.... ·179;		
		....	– ·005.
In 1881, 5·75...... ;	.... ·174.		

II. Distance from the cornea of the left eye at which a luminous point presents the appearance of a nearly vertical line.

In 1825, 6·0 inches;	Reciprocal = ·166;	Difference =	− ·054.
In 1846, 8·9 ......;	.... ·112;	....	− ·018.
In 1866, 10·6 ......;	.... ·094;	....	+ ·006.
In 1871, 10·0 ......;	.... ·100;	....	− ·027.
In 1881, 13·75 ......;	.... ·073.		

III. Measure of the astigmatic power of the left eye at different epochs; estimated in each case by the difference of the reciprocals for the same date in the two preceding tables.

In 1825,	Astigmatism = ·120;	Difference =	− ·019.
In 1846,	.... ·101;	....	− ·010.
In 1866,	.... ·091;	....	− ·012.
In 1871,	.... ·079;	....	+ ·022.
In 1881,	.... ·101.		

IV. Distance from the cornea of the right eye, at which a luminous point is seen distinctly.

In 1846, 4·7 inches;	Reciprocal = ·213;	Difference =	− ·031.
In 1866, 5·5 ......;	.... ·182;	....	+ ·003.
In 1871, 5·4 ......;	.... ·185;	....	− ·018.
In 1881, 6·0 ......;	.... ·167.		

The image formed by the right eye is very perfect; although there are anomalous spots on the cornea or crystalline, and appearances which suggest a fault in the retina.

(3) *On the mechanism of the renal secretion.* By C. S. Roy, M.D.

The observations which formed the basis of this communication were made in part during the summer session of last year in conjunction with Professor Cohnheim, at the Leipzig Pathological Institute, and in part during the last autumn and winter in the physiological laboratory of this University. They were commenced with the hope of being able to elucidate a number of questions bearing upon the relation which exists between certain diseases of the kidney and cardiac hypertrophy.

It was first sought to obtain information upon this subject by investigating the manner and extent to which the action of the heart is affected by obstruction of the renal arteries and the other large branches of the aorta; the facts obtained by taking this line

of inquiry were not however of a kind fitted to throw light upon the problem which it was specially desired to sôlve.

It soon became evident that an investigation of the manner in which the renal secretion and circulation are normally regulated, and the relation which these bear to the regulating mechanism of the systemic circulation would be best fitted to supply information of the kind required.

The method employed was, to record graphically the changes in volume of one or both kidneys, while at the same time the changes in the blood-pressure in the aorta and the rapidity with which the urine was secreted were also recorded on the same revolving cylinder or, as continuous tracings, upon the paper of Ludwig's kymograph.

The method used for recording the changes in volume of the kidney is the same in principle as that of the plethysmograph. The kidney is enclosed in a rigid metal box, the arrangement being such that while the organ can freely expand or contract, and while the changes in volume are recorded by a lever writing with a light glass pen upon the kymograph paper, no obstruction is offered to the entrance and exit of blood by the renal vessels nor to the outflow of urine by the ureter. The kidney is surrounded by warm olive oil, which, however, is not in immediate contact with its surface, but is separated from it by a delicate flexible membrane of a kind which has already been referred to by the author in several of his published papers, and which prevents any escape of the oil by the side of the blood-vessels and other structures entering the hilus of the gland. It is impossible, consistently with the brevity desirable in a communication of this kind, and without the aid of a diagram, to describe in a satisfactory manner the exact arrangement of the parts of the instrument, and the reader is referred for the complete account of the method used to a paper which will shortly be published in conjunction with Professor Cohnheim giving an account of the first part of these observations. At present it must suffice to say that, when the instrument is in use, the kidney lies between two delicate, exceedingly flexible membranes, which apply themselves closely to its surface and to the surface of the structures entering the hilus of the organ, and that each of these membranes forms with each of the symmetrical halves of the box a chamber which is filled with oil and which communicates by a relatively wide flexible tube with the recording instrument.

The metal box is roughly kidney-shaped, and the two symmetrical halves, the edges of which meet in a plane corresponding to the long axis and the hilus of the kidney, are joined together by a hinge. Opposite the hinge each half of the box has a semicircular incision cut into it, and these together form a round hole through which pass the structures entering the hilus.

The recording instrument proper resembles in many respects an arrangement which was described by the author in a paper which appeared recently in the *Journal of Physiology* upon the form of the pulse-wave.

It need not be described here, and it must suffice to mention that this instrument permits of even rapid changes in the volume of the kidney being recorded without its producing the slightest change in the pressure of the fluid by which the kidney is surrounded. It allows also the numerical value of changes in volume being ascertained with exactness.

The rapidity of the flow of urine is followed with great convenience and accuracy by an arrangement by which each drop of urine, which flows from a narrow tube tied in the ureter, falling upon a light aluminium plate at the end of a well balanced lever, causes, by its impact, a momentary descent of the latter, which dips the point of a fine platinum wire into a mercury cup, closing thereby for an instant a galvanic current, and causing a mark to be made upon the paper of the kymograph by means of an electro-magnetic marker.

Each tracing then shews, 1st. the changes in volume of the kidney, 2nd. the aortic blood-pressure, 3rd. the rapidity with which the urine is being secreted, 4th. the time (which was recorded by a seconds pendulum and an electro-magnetic marker in the ordinary way).

The operation is the same as that for nephrotomy, the kidney being reached from the lumbar aspect. It is cleared of all its connections leaving only intact the structures entering its hilus. It is then enclosed in the metal box which has previously been warmed, and the two compartments of which are now filled with warm olive oil which fills also the flexible tube connecting the box with the recording instrument. It need scarcely be added that the animal,—rabbit, cat or dog, in most cases the latter, was kept fully under the influence of ether, chloroform or morphia, or a combination of two of these, from the commencement to the end of the experiment.

Contrary to what might reasonably have been anticipated, the kidney continues to secrete urine of normal quality and in quantity more or less exactly the same as that of the intact organ of the other side, for many hours after it has been placed in the metal box.

The following brief and incomplete *resumé* may serve to indicate the nature of the principal facts arrived at by Professor Cohnheim and the author in Leipzig, or by the author independently in Dr M. Foster's laboratory.

1. The volume of the kidney varies with each pulse-wave and with the respiration curves of the blood-pressure;—the tracing obtained resembling closely that of the mercurial kymograph.

2. The renal vessels are exceedingly elastic, a rise or fall of the aortic blood-pressure causing an expansion or contraction of the kidney which may be very considerable in amount, *e.g.* in one case a rise of the blood-pressure from 70 to 180 mm. of mercury (brought about by closing the innominate and carotid arteries and clamping the aorta below the point at which the renal arteries are given off) caused an expansion of the kidney which was equal to 10 per cent. of the volume which the organ was found to measure after death.

3. The changes in volume of the two kidneys are not necessarily identical either in quantity or in direction—one kidney, for example, may be slowly expanding while the other remains constant in volume or is even slowly contracting; further, any influence which causes contraction or expansion of the renal vessels need not cause expansion or the opposite to an equal extent in both glands.

4. After the immediate effect of the operation has passed off, the volume of the kidney will usually remain unchanged (with exception of the changes due to the pulse and respiration) for many hours unless some change in the conditions of the experiment be intentionally introduced.

5. When the *Traube-Hering* curves of the blood-pressure present themselves, the volume of the kidney does not expand with the rhythmic rise in the blood-pressure. With each rise of the blood-pressure the kidney *contracts*, expanding with each fall of the blood-pressure. The renal vessels are, therefore, amongst those to the rhythmic contraction and expansion of which the *Traube-Hering* waves are due.

6. Arrest for 3 or 4 minutes of the artificial respiration, where that is employed, and where curare has been previously injected, causes a contraction of the renal vessels (which may reach 12 per cent. of the post-mortem volume of the kidney) simultaneously with the rise of aortic blood-pressure which is produced by the asphyxia.

7. Stimulation of the medulla oblongata by weak induced currents causes a powerful contraction of the renal vessels.

8. Stimulation of the central end of a sensory nerve, *e.g.* sciatic, brachial plexus, splanchnic, &c. causes a contraction of the renal vessels simultaneous with the rise in the aortic blood-pressure.

Stimulation of the central end of the vagus causes a contraction of the kidney (where the vagus of the other side has been cut to eliminate reflex inhibition of the heart), and the renal vessels *contract* whether the stimulation of the central end of the vagus cause a rise or a fall of the aortic blood-pressure.

9. Stimulation of nearly all the roots of the splanchnic in the thorax, and of both larger and smaller splanchnic nerve-trunks causes contraction of the kidney of the *opposite* side.

The extent to which the kidney contracts on stimulation of the splanchnic is usually very considerable. In one case the kidney contracted on stimulating with a strong induced current for three minutes to an extent which was equal to 63 per cent. of the post-mortem volume of the organ.

10. In nearly every case stimulation of the peripheral end of the cut splanchnic at the point where it passes through the diaphragm causes contraction of *both* kidneys; the kidney of the side opposite to the nerve stimulated commencing to contract later than the one on the same side as the stimulated nerve.

11. Stimulation of the central end of a sensory nerve, or of the medulla oblongata, or of the cervical spinal cord, causes a contraction of the renal vessels after *both* splanchnics have been cut at their point of entrance into the abdominal cavity.

Vaso-constrictor influences may therefore pass from the spinal cord to the kidney by some other path than the two splanchnics.

12. Section of the splanchnic does not always cause an expansion of the renal vessels, a fact which would make it doubtful whether a vascular tonus of the renal vessels emanating from the vasomotor centre or centres in the spinal cord is normally present.

13. Stimulation of the *central* ends of the majority of the fine nerves which enter the kidney along with the vessels causes a contraction of the vessels of the kidney.

14. Stimulation of the *peripheral* end of each and all of the renal nerves which accompany the vessels causes a contraction of the organ.

15. After section of all but one of the (usually from 7 to 11) nerves accompanying the renal vessels, stimulation of the peripheral end of the splanchnic or of a sensory nerve still causes a contraction of the kidney which differs but little in amount from that produced by the same stimulation when all the renal nerves were intact, but which takes longer time to shew itself after the stimulation.

16. The latent period between the commencement of the stimulation and the contraction which it produces may vary greatly in different individuals and in the same individual under different conditions.

17. Great differences may exist in the length of the latent period according to the strength of the stimulation employed, but

still more so according as it is applied to a splanchnic nerve or to a sensory nerve, *e.g.* for the peripheral end of the splanchnic it may be less than half a second while the same current applied to the central end of the sciatic of the same animal may give a latent period of three seconds or even very much longer.

18. As a rule it is difficult to sever completely all the nerve fibres which accompany the renal vessels so that stimulation of the peripheral end of the splanchnic will no longer cause a constriction of the vessels of the kidney, but great differences in this respect occur in different animals. While with some animals, section of all the nerve trunks which are visible to the naked eye will suffice for this purpose, with others it is only after the most laborious and minute cleaning of the walls of the vein and artery that vasoconstrictor influences no longer pass to the kidney on stimulation of the splanchnic.

19. When these nerves have been completely severed stimulation of the peripheral end of the cut splanchnic no longer causes a contraction but an expansion of the renal vessels which goes hand in hand with the rise of the blood-pressure.

20. The secretion of urine increases or diminishes *caeteris paribus* with the degree of expansion of the renal vessels.

21. The renal vessels expand or contract with great readiness to relatively slight changes in the chemical constitution of the blood. For example, the injection into the veins of even a very small quantity of water (1-2 c.cm. in the case of a medium-sized dog) causes a contraction of the kidney varying in amount and lasting from 1-2 seconds to 2-3 minutes, and which is succeeded by a more or less well marked expansion of the kidney lasting for a much longer time than the contraction.

22. Injection of a small quantity of $\frac{1}{2}$ per cent. salt solution causes a primary expansion of the renal vessels varying in degree with the amount of the fluid injected, with the individual, and with the condition of the individual at the time of injection.

23. Urea in $\frac{1}{2}$ per cent. or in 5 or 10 per cent. solution causes a primary constriction of the renal vessels followed by a more or less powerful expansion of longer duration. Digitalis causes also a primary contraction, differing however from urea in the fact that this contraction is of longer duration, but resembling it in so far that it is followed by a secondary expansion. A certain number of other diuretics resemble water, urea and digitalis in causing a primary contraction with diminution or arrest of secretion, followed by expansion and increased secretion.

24. Other diuretics again, such as nitrate of soda, acetate of

potash, resemble salt solution in causing a primary expansion of the renal vessels.

25. Injection into the veins of defibrinated blood or serum of the same species causes only an expansion of renal vessels when an evident increase in the blood-pressure is produced by the quantity injected.

26. All the diuretics mentioned, when they are given in not too large a dose, cause a very considerable change in the volume of the kidney without causing any change in the blood-pressure.

27. All of the diuretics mentioned have exactly the same influence upon the renal vessels when injected into the blood *after complete section of the nerves which accompany the renal vessels.* Their action is, therefore, not due to any vasomotor influence transmitted from the spinal centres, but is due to the influence of the change in constitution of the blood either upon the walls of the renal vessels directly, or upon some peripheral vaso-regulating mechanism of a nervous nature contained in the kidney itself. The evidence in favour of one or other of these views would be out of place in a preliminary communication such as this.

PROCEEDINGS

OF THE

Cambridge Philosophical Society.

ANNUAL GENERAL MEETING.

October 31, 1881.

Professor Newton, President, in the Chair.

The following were elected Officers and new members of Council for the ensuing year:

President.

Mr F. M. Balfour.

Vice-Presidents.

Dr W. M. Campion.
Professor Babington.
Professor Newton.

Treasurer.

Dr J. B. Pearson.

Secretaries.

Mr J. W. Clark.
Mr Coutts Trotter.
Mr J. W. L. Glaisher.

New members of Council.

Professor Liveing.
Mr Michael Foster.
Mr R. T. Glazebrook.

W. Hillhouse, B.A., Trinity College, and A. Hill, B.A., Downing College, were balloted for and duly elected Fellows of the Society.

Professor Newton on leaving the chair made the following remarks:

On laying down the office by which I have been honoured for the past two years I hope you will allow me to say a few words. At our last Annual General Meeting I ventured to direct the attention of the Society to three subjects which seemed to be especially worthy of its consideration, and to these I would again refer. There was first an alteration in our hour of meeting. The suggestion I then threw out was favourably received by the Society, and I think no one regrets the change, which was in consequence made; while I know that to the officers and Council it has proved a matter of great convenience. There was next the question of the removal of our Library. This also met with the approval of the Society, and what is more with that of the University at large. I have to tender my sincere thanks to all who have aided me in the somewhat protracted negotiations that were necessary for carrying out a scheme which I believe will be most beneficial to the progress of the scientific studies of the University, and I am bound to add that our proposals have throughout been met in a most liberal spirit by the Senate. The joint Committee appointed by the Council of the Society and by the Museums and Lecture Rooms Syndicate to manage the 'Cambridge Philosophical Library' on Friday last agreed to the set of Rules a copy of which I now place on the table, and I have the pleasure of announcing that the Library is not only open, but in working order. I hope it will be admitted that in framing these Rules, the representatives of the Society have duly cared for the interests of its Fellows, and that the inconveniences to which they will at times be subjected, through the room being occasionally required for examinations, will in practice prove to be but slight, and more than compensated by the advantage of our books being rendered far more accessible than they were before, and being placed under the direct charge of a Library-clerk. In this connexion I must mention the name of one of our Secretaries, Mr J. W. Clark, whose services in the very considerable operations of removing, rearranging and recataloguing our books have been unremitting, and the Council by passing a special vote of thanks to him on that account has endeavoured to acknowledge the obligation under which the Society—I may say the University—lies. Thirdly I have to refer to the recovery and restoration to its proper place of the Society's lost Charter, which it was my agreeable duty to lay before you at our meeting on the 21st of March.

I have now only once more to express my gratitude to the Council and all the Fellows of the Society—but especially to its officers—for the able and kind assistance they have rendered me

during the past twelve months—as during the preceding year; and I trust that my Presidency which closes to-day will not in future be thought to have been detrimental to the prosperity of the Society.

Mr BALFOUR having taken the chair the following communication was made to the Society:

Note on Abel's Theorem. By Professor CAYLEY.

Considering Abel's theorem in so far as it relates to the first kind of integrals, and as a differential instead of an integral theorem, the theorem may be stated as follows:

We have a fixed curve $f(x, y, 1) = 0$ of the order m; this implies a relation $f'(x)\,dx + f'(y)\,dy = 0$, between the differentials dx, dy of the coordinates of a point on the curve; and we may therefore write

$$d\omega = \frac{dx}{f'(y)} = -\frac{dy}{f'(x)},$$

and, instead of dx or dy, use $d\omega$ to denote the displacement of a point (x, y) on the curve.

Taking for greater simplicity the fixed curve to be a curve without nodes or cusps, and therefore of the deficiency $\frac{1}{2}(m-1)(m-2)$, we consider its mn intersections by a variable curve $\phi(x, y, 1) = 0$ of the order n. And then, if $(x, y, 1)^{m-3}$ denote an arbitrary rational and integral function of (x, y) of the order $m-3$, the theorem is that we have between the displacements $d\omega_1$, $d\omega_2 \ldots d\omega_{mn}$ of the mn points of intersection, the relation

$$\Sigma\,(x, y, 1)^{m-3}\,d\omega = 0,$$

where the left-hand side is the sum of the values of $(x, y, 1)^{m-3}\,d\omega$, belonging to the mn points of intersection respectively.

For the proof, observe that, varying in any manner the curve ϕ, we obtain

$$\frac{d\phi}{dx}\,dx + \frac{d\phi}{dy}\,dy + \delta\phi = 0,$$

where $\delta\phi$ is that part which depends on the variation of the coefficients, of the whole variation of ϕ; viz. if $\phi = ax^n + bx^{n-1}y + \ldots$, then $\delta\phi = x^n da + x^{n-1}y\,db + \ldots$, $\delta\phi$ is thus, in regard to the coordinates (x, y), a rational and integral function of the order n. Writing in this equation

$$dx,\, dy = \frac{df}{dy}\,d\omega,\ -\frac{df}{dx}\,d\omega,$$

the equation becomes

$$\left(\frac{d\phi}{dx}\frac{df}{dy}-\frac{d\phi}{dy}\frac{df}{dx}\right)d\omega+\delta\phi=0,$$

or say $$-J(f,\phi)\,d\omega+\delta\phi=0,$$

that is $$d\omega=\frac{\delta\phi}{J(f,\phi)};$$

and then multiplying each side by the arbitrary function $(x, y, 1)^{m-3}$, we have

$$\Sigma\,(x, y, 1)^{m-3}\,d\omega=\Sigma\,\frac{(x, y, 1)^{m-3}}{J(f,\phi)}\,\delta\phi,$$

where $\delta\phi$ being of the order n in the variables, the numerator is a rational and integral function of (x, y) of the order $m+n-3$: hence by a theorem contained in Jacobi's paper *Theoremata nova algebraica circa systema duarum æquationum inter duas variabiles*, Crelle t. xiv. (1835) pp. 281—288, the sum on the right-hand side is $=0$: hence the required result $\Sigma\,(x, y, 1)^{m-3}d\omega=0$.

Observing that $(x, y, 1)^{m-3}$ is an arbitrary function, the equation just obtained breaks up into the equations

$$\Sigma d\omega=0,\ \Sigma x d\omega=0,\ \Sigma y d\omega=0,\ldots\ \Sigma x^{m-3}d\omega=0,\ldots\ \Sigma y^{m-3}d\omega=0,$$

viz. the number of equations is

$$1+2+\ldots+(m-2),\quad =\tfrac{1}{2}(m-1)(m-2),$$

which is $=p$, the deficiency of the curve.

Suppose the fixed curve $f(x, y, 1)=0$ is a cubic, $m=3$, and we have the single relation $\Sigma d\omega=0$, where the summation refers to the $3n$ points of intersection of the cubic and of the variable curve of the order n, $\phi(x, y, 1)=0$.

In particular if this curve be a line, $n=1$, and the equation is $d\omega_1+d\omega_2+d\omega_3=0$; here the two points (x_1, y_1), (x_2, y_2) taken at pleasure on the cubic, determine the line, and they consequently determine uniquely the third point of intersection (x_3, y_3); there should thus be a single equation giving the displacement $d\omega_3$ in terms of the displacements $d\omega_1$, $d\omega_2$; viz. this is the equation just found $$d\omega_1+d\omega_2+d\omega_3=0.$$

So if the variable curve be a conic, $n=2$; and we have between the displacements of the six points the relation

$$d\omega_1+d\omega_2\ldots+d\omega_6=0:$$

here five of the points determine the conic, and they therefore determine uniquely the sixth point; and there should be between the displacements a single relation as just found.

If the variable curve be a cubic, $n=3$, and we have between the displacements of the nine points the relation

$$d\omega_1 + d\omega_2 \ldots + d\omega_9 = 0:$$

here eight of the points do *not* determine the cubic ϕ, but they nevertheless determine the ninth point, viz. (reproducing the reasoning which establishes this well-known and fundamental theorem as to cubic curves) if $\phi_0 = 0$ be a particular cubic through the 8 points, then the general cubic is $\phi_0 + kf = 0$, and the intersections with $f=0$ are given by the equations $\phi_0 = 0$, $f=0$; whence the ninth point is independent of k, and is determined uniquely by the 8 points. There should thus be a single relation between the displacements, viz. this is the relation just found.

And so if the variable curve be a quartic, or curve of any higher order, it appears in like manner that there should be a single relation between the displacements; this relation being in fact the foregoing relation $\Sigma d\omega = 0$.

But take the fixed curve to be a quartic, $m=4$: then we have between the displacements $d\omega$ the relation

$$\Sigma\,(x,\ y,\ 1)\,d\omega = 0,$$

that is the three equations

$$\Sigma x d\omega = 0, \quad \Sigma y d\omega = 0, \quad \Sigma d\omega = 0.$$

If the variable curve is a conic, $n=2$, then there are 8 points of intersection; 5 of these taken at pleasure determine the conic, and they consequently determine the remaining 3 points of intersection: hence there should be 3 equations. And so if the variable curve be a curve of any higher order, then by considerations similar to those made use of in the case where the first curve is a cubic it appears that the number of equations between the displacements $d\omega$ should always be $=3$.

But if the variable curve be a line, $n=1$, then the number of the points of intersection is $=4$: 2 of these taken at pleasure determine the line, and they consequently determine the remaining 2 points of intersection; and the number of equations between the displacements $d\omega$ should thus be $=2$. But by what precedes we have the 3 equations

$$\begin{aligned} d\omega_1 + \ d\omega_2 + \ d\omega_3 + \ d\omega_4 &= 0, \\ x_1 d\omega_1 + x_2 d\omega_2 + x_3 d\omega_3 + x_4 d\omega_4 &= 0, \\ y_1 d\omega_1 + y_2 d\omega_2 + y_3 d\omega_3 + y_4 d\omega_4 &= 0; \end{aligned}$$

here the 4 points of intersection are on a line $y = ax + b$; we have therefore $y_1 = ax_1 + b, \ldots y_4 = ax_4 + b$; the equations between the

$d\omega$'s give $(y_1 - ax_1 - b_1)\, d\omega_1 + \ldots + (y_4 - ax_4 - b)\, d\omega_4 = 0$, that is, is a single relation $0 = 0$; or the 3 equations thus reduce themselves to 2 independent equations.

Again, if the fixed curve be a quintic, $m = 5$, there are here between the displacements the 6 equations

$$\begin{array}{lll} \Sigma x^2 d\omega = 0, & \Sigma xy d\omega = 0, & \Sigma y^2 d\omega = 0, \\ \Sigma x d\omega = 0, & \Sigma y d\omega = 0, & \Sigma d\omega = 0; \end{array}$$

the two cases in which the number of independent equations is less than 6 are 1° when the variable curve is a line, and 2° when the variable curve is a conic. For the line $n = 1$, and the number should be $= 3$. We have the above 6 equations; but the equation of the line is $ax + by + c = 0$, that is, we have $ax_1 + by_1 + c = 0$, &c.; we deduce the 3 identical equations

$$\Sigma x\,(ax + by + c) = 0, \quad \Sigma y\,(ax + by + c) = 0, \quad \Sigma\,(ax + by + c) = 0,$$

and the number of independent equations is thus $6 - 3, = 3$ as it should be.

So when the variable curve is a conic, $n = 2$; the number of independent equations should be $= 5$. The points of intersection lie on a conic $(a, b, c, f, g, h \between x, y, 1)^2 = 0$; we have therefore the several equations $(a, b, c, f, g, h \between x_1, y_1, 1)^2 = 0$, &c.: we have therefore the single identical equation

$$\Sigma\,(a, b, c, f, g, h \between x, y, 1)^2\, d\omega = 0,$$

and the number of independent equations is $6 - 1, = 5$ as it should be.

Obviously the like considerations apply to the case where the fixed curve is a curve of any given order whatever.

November 14, 1881.

Mr F. M. Balfour, President, in the Chair.

The following communication was made to the Society:

On the rocks of the Channel Islands, No II. By Professor Liveing.

In a former communication on this subject I drew from my observations of the rocks of the Channel Islands, principally those of Guernsey, the conclusion that the granitic structure is a metamorphic character which may be imparted either to stratified or to igneous rocks, which is not due to igneous fusion, but is rather the result of continued variations of temperature always far short

of fusion, assisted by the action of water or steam and perhaps other gases. Further observations made during last summer, chiefly on the rocks of Serk and Jersey, tend to confirm the conclusions previously arrived at, and at the same time bring out some fresh points which appear to be of sufficient interest to lay before the Society.

On Guernsey I have little to add to what I said before, except to point out that in the section which I gave, the strata should have been represented as bending downwards on the S. side far more abruptly so as to become nearly vertical in that part of the island.

The geology of Serk is very simple, at the same time very characteristic, so that it is an excellent commentary on that of the other Channel Islands. Serk is a table-land elevated about 400 feet above the sea-level. On the East and West sides, that is the sides which form the longer dimensions of the island, the table ends in precipitous cliffs, but at the North and South ends it falls more gradually to the sea, ending in long promontories continued for some distance by detached rocks. The island of Brecqhou forms a similar prolongation with a gradual slope to the sea on the West side, and may really be considered as part of Serk, from which it is separated by the narrow Gonliot pass only about 80 yards wide. The cliffs are on all sides intersected by deep ravines, sometimes by chasms with sides absolutely vertical, and are perforated at the sea-level by numerous, often deep, caves. These features are, of course, the natural consequences of its geological structure. The mass of the island is formed of a crystalline hornblende schist cleaving readily parallel to the bedding, and making an excellent building material in everything but its colour which is coal-black. This schist rests, as seen at one place only, namely on the shore at the Port du Moulin on the N.W. side, on a stratum of grey syenite or syenitic gneiss. Upwards the schist passes into a more compact rock consisting of alternate laminæ of hornblende and of felspar mixed with a little quartz. This again passes rather abruptly into a grey syenite in which the bedding can be traced for a short distance upwards and is then lost for a considerable thickness. The greater part of little Serk is composed of this granitic rock closely resembling that of the extreme north of Guernsey, and not unlike that of Herm which supplied the steps at the foot of the Duke of York's column in London. The originally stratified character of this syenite is however noway doubtful, for the bedding can be traced in it in many places, and at Port Goury in the South end of little Serk a thin bed in it crops out which has a slightly different mineral character, is distinctly bedded, and being pervious to water has undergone more decomposition than the rock above and below it, and

has thereby produced a fall of the superjacent rock leaving a platform about 20 yards wide with a gentle slope seaward in the plane of the bedding.

The centre of elevation has been at the Port du Moulin above mentioned: there the strata are upraised to the greatest height, and are almost horizontal, and from that point they dip away in all directions. The main line of dislocation appears to run from this point across the island to the Creux harbour on the east side, and has produced the ravine extending from the Port du Moulin up the grounds of the Seigneurie, and that down which the road runs to the Creux harbour. South of this line the strata dip away towards the S.W. Their inclination at first is slight but gradually increases to about 40° at the Coupée, as the isthmus between Great and little Serk is called, and then diminishes again to about 15° (measured by the eye only) at the southern end of little Serk. To the north of the principal line of dislocation the strata dip with a gradually increasing slope in a radial manner from N.W. round to East. Northwards the schist dips under the syenite, and the junction may be seen on the shore on the N.E. side near the foot of the wall as you descend to the Epercherie landing place, and on the N.W. side at a point nearly midway between the Boutiques caves and the Autelets. On the west the schist dips under the syenite near the extremity of Brecqhou. On the south the junction may be seen on the shore of the Grande Gréve a little east of Point le Jeu, and a little north of the Moie on the other side of Little Serk, both on the shore and in High Cliff above. It is the great toughness and power of resistance of the grey syenite which has preserved the projecting extremities of the island at Little Serk, Brecqhou and Bec du Nez.

The island is everywhere intersected by volcanic dykes mostly vertical, and in many places also by veins of which one in Little Serk is metalliferous, and was at one time worked for lead and silver, but others are mostly mere cracks which have in places a lining of quartz and have otherwise been filled up by debris from the sides, and through the facilities they afford for the action of the weather and the sea, have produced deep indentations of the coast. The largest vein of this kind has thus given rise to the Coupée, another to the Havre Gosselin. The volcanic dykes are mostly greenstone, they are largest and most frequent on the N.W. side, about the area of greatest elevation, and as their materials are much more easily disintegrated than the tough hornblende rock and syenite they have frequently been removed by the action of the waves and weather, and left the deep chasms and caves before alluded to. One of the most remarkable of the caves runs right through the promontory called Moie des Moutons. The dyke there is about 16 feet thick, and the lower part of it as high

as the sea can reach is completely removed while the upper part is untouched, so that the result is a long gallery with vertical sides and perfectly horizontal roof. The dykes are however not all of greenstone or so easily disintegrated. One of an exceedingly beautiful porphyry with crystals of white felspar disseminated through it, cuts into the northern promontory a little south of the Boutiques. It resists weather equally well with the syenite, and pebbles of it are found all round the coast. It no doubt belongs to an eruption of a date different from that which elevated the Port du Moulin.

After having observed the rocks in Serk I have no doubt that the syenite is a part of the same stratified system with the hornblende schist: and further that it has acquired its highly crystalline character and lost more or less its bedded appearance by a process which has failed to obliterate the stratification of the subjacent schist, and therefore can hardly have been fusion or any near approach to a fused state. There is no need to suppose even that the syenite and schist have been exposed to any different circumstances, the latter is highly crystalline as well as the former, but the former may have been originally deposited in a more homogeneous state. In the crystallizing action, as it goes on in substances so solid as ordinary rocks, the transport of material from one part of the mass to another can take place but very slowly indeed, so that the complete obliteration of stratification in rocks formed of alternating layers of different materials requires a very long time, much longer than in rocks of which the successive layers were of uniform or nearly uniform composition.

The Serk beds give strong evidence in confirmation of the view which I have previously taken of the metamorphic character of the Guernsey syenite.

In Jersey the granitic rocks have mainly determined the form of the island. On the S. the masses projecting seaward are granitic and between them the more easily disintegrated volcanic ashes and trap, of which the central part of the island consists, have been washed away, forming St Aubin's and St Brelade's Bays. On the N. the whole coast from a little W. of Fremont Point to Grosnez Point is granitic. Between this and the syenite of the Corbière Point, St Ouen's Bay has been denuded out of volcanic ashes and partly refilled with blown sand. On the E. the projecting granitic boss of Mont Orgueil has protected the land behind it, but the volcanic rocks have again been scooped out in Grouville Bay. Fremont Point is a very tough trap, and the whole of the N.E. projection of Rozel between Bouley Bay and St Catharine's Bay consists of a hard conglomerate of rolled pebbles, apparently an old sea-beach, cemented with volcanic ashes. This resists weather well, and moreover dips seawards,

and is thereby the better able to withstand the action of the waves.

It is difficult to trace any connection between the rocks of Jersey and those of the other Channel Islands notwithstanding the short distance which separates them. The Jersey syenite on the N. side, nearest Serk, is very unlike the Serk syenite, it is generally more quartzose and contains less hornblende; in some places the felspar in it is bright red in others nearly white, in some places there is mica, in others none, and in the most inland quarries in St Lawrence's parish it becomes almost a quartzite consisting mainly of quartz with only a small proportion of felspar and little or nothing else. I have not seen any definite signs of bedding in it. On the S. side of the island the syenite consists chiefly of a coarsely crystalline mixture of hornblende and felspar, and may be a highly metamorphosed condition of the Guernsey gneiss; but no bedding can be traced in it unless some more quartzose veins which extend like thin beds for some distance in it represent different beds of the original deposit. If that be so the bedding is nearly vertical as it is at the S.E. of Guernsey. The boss of fine grained red syenite on which Fort Regent stands has all the appearance of being eruptive, and dykes of similar composition but not quite the same character of crystallization, which cut through the coarser syenite of the rising ground on which Victoria College stands, may be connected with it. The rocks on the shore at the Collinette under the fort and all along Grève d'Azette as far as Le Hocq Point in St Clement's Bay, consist of a grey syenitic greenstone consisting of hornblende and felspar, tolerable fine in grain, and no doubt, I think, eruptive. Grey dykes of a similar material but different texture cut through the S. end of the boss of red syenitic rock of Fort Regent, which appears therefore to be of older date and unconnected with the volcanic rocks mentioned hereafter.

In the view which I have advanced as to the crystallization of granitic rocks there is nothing to limit this kind of metamorphism to aqueous rocks. The resulting character of the rock will be determined by the nature and proportions of the materials of the original rock, and upon their mode of aggregation and upon the permeability of the mass by steam and other gases. My view is supported by the varying character of the crystallization of this greenstone rock; a character which is common enough in greenstones elsewhere. Here it is mostly somewhat fine grained though the component minerals are quite distinct, but in places where the former existence of rather wide joints is traceable it has become coarsely crystalline, there has been in those places a greater segregation of materials until the crystals have grown to be an inch or more in length. The rock in such places re-

sembles that at similar spots in the Guernsey syenite. An instance of this kind of segregation is seen at the boss forming Le Nez Point on the W. side of St Clement's Bay.

I have before called attention to the curious metamorphism by which a stratified rock has taken a columnar jointing like basalt at La Crête Point in St Catharine's Bay. A similar columnar jointing in the same rock is also seen inland in the valley leading from St Martin's towards St Catharine's Bay only a few hundred yards from the Church. These and all the stratified rocks of the island I believe to be volcanic ashes. In some places they are quite slatey, and in my former communication I mentioned them as shales and slates. In the N.E. of the island these ashes are of a peculiar reddish-purple colour, compact, and in many places regularly stratified, and in different spots shew varying degrees of crystallization. At some spots the stratified structure seems quite effaced while in the same mass at a very short distance the stratification is still quite plainly seen. In some places there has been an approach to a porphyritic segregation of materials, and in others the rock has acquired completely the porphyritic character and looks like a trap rock, but the continuity and gradual sequence in crystalline character leave no doubt in my mind that the whole is metamorphosed ash. In the central part of the island the purple colour gives way to a grey, but the same gradual segregation of the felspar in crystals may be traced in it in some parts, as in the valley leading down to the town mills in St Saviour's parish. In some places the felspar crystals are of two colours, red and green. This I take to be due to some difference in the chemical composition of fragments in the original ashes which have served as nuclei for the crystallization and supplied some of the substance of the crystals. There are however masses of trap amongst the ashes in some places. Under Gallows hill, where is now the People's Park, such a mass has been largely quarried.

At the junction between the granitic and volcanic rocks on the N. side of the island between Cotil and Fremont Points the granite is of a bright red colour and appears at first sight to have sent branching veins of a similar red colour into the adjacent rock, looking very much as if the granite had been forced into cracks in a fused state. Any such inference would in this case be very rash. It would imply that the granite is a newer formation than the volcanic rock into which it is intruded, which cannot be the case for the following reasons. In the first place the granite at the junction is highly quartzose and must have a far higher fusing point than the highly felspathic rock adjoining, so that if the fused granite had been brought in contact with the felspathic rock the latter must have been more or less fused up

with the granite at the junction. This however is not the case; the junction is sharply defined and in the cliff the felspathic rock has weathered away for some distance leaving a clean wall of granite. On the W. side where the junction occurs between the granite and stratified ashes, seen about half a mile from L'Etacq on the St Ouen's road, the ashes rest on what looks to me like an eroded surface of granite. The ashes may however be of a different date from the rock of Fremont Point, though I see no ground for thinking that there can be much difference of date, as the Fremont Point rocks are evidently part of one system with the other rocks of Bonne Nuit Bay, which are continuous with those in the interior of the island. I rest my conclusion however, as to the greater antiquity of the granite on another ground, which is, that the granite is intersected by many dykes, mostly greenstone, while I have not detected a single greenstone dyke, only one or two dykes of any kind, in the ashes and trap which form the central part of the island. The rocks of Fort Regent and Grève d'Azette before mentioned are as full of dykes as the granite but are disconnected by their mineral characters from the system of volcanic rocks of the interior of the island. I conclude therefore that these rocks, as well as the granitic, are of older date than the intrusion of the dykes, but the volcanic rocks of the rest of the island newer. If that is so, we must look to some cause other than igneous fusion for filling the veins. In this connection it is worth notice that in most cases, all that I have ever examined, the materials which fill branching veins of granite differ somewhat from those of the mass from which they appear to be derived; generally they are more quartzose, and I have, in my former paper, noticed the peculiarity in the arrangement of the felspar crystals in some of them. If these Jersey veins had been purely quartz veins they would be set down at once as results of segregation from the adjoining rocks, and if they had been purely felspathic, occurring as they do in a felspathic rock, they would be common occurrences assigned to a similar cause. In this case we have a mixture of quartz and felspar in which the quartz predominates and is coloured red with oxide of iron like the adjoining granite, and there is no reason to assume that any different agency has been at work in producing it, from those which produce ordinary quartz and felspar veins. It is worth notice that the porphyritic crystals of felspar in the trap rock are also coloured red for a short distance from the granite; and in Guernsey I have seen, near Cobo, a vein of red felspar passing right across a dark coloured greenstone dyke from wall to wall of red granite, where the granite seems at least to have supplied the colouring matter if not the mass of the vein. It must not be supposed that I deny the possibility of

granite being produced from a rock which has undergone fusion. What I hold is that the crystalline character of granite is a result of metamorphosis. If the veins were intruded in a fused state into crevices in a more fusible rock already solidified, they must from the predominance of quartz in them have solidified at once into a glassy mass which afterwards can only have become devitrified and the materials segregated into comparatively large crystals by a process which goes on very slowly indeed, and would be almost certainly slower in a vitrified than in a more porous mass.

November 28, 1881.

Mr F. M. Balfour, President, in the Chair.

Mr W. L. Mollison, M.A., Clare College, Mr W. B. Allcock, Emmanuel College, and Mr H. T. Stearn, M.A., King's College, were balloted for and duly elected Fellows of the Society.

The following communications were made to the Society:

(1) *Celestial Chemistry from the time of Newton.* By T. Sterry Hunt, LL.D., F.R.S.

The late W. Vernon Harcourt, in 1845[1], called attention to the remarkable perception of great chemical truths which is apparent in the Queries appended to the third book of Newton's *Optics*, as well as in his Hypothesis touching Light and Colour. With regard to the latter, Harcourt then remarked, "it has, I think, scarcely been quoted, except by Dr Young, and its existence is but little known, even among the best informed scientific men." The essay in question was read before the Royal Society, December 9th and 16th, 1675, but remained unpublished till 1757, when Birch, at that time secretary to the Society, printed it, not without verbal inaccuracies, in the third volume of his *History of the Royal Society*, a work intended to serve as a supplement to the *Philosophical Transactions* up to that date. In 1846, at the suggestion of Harcourt, the Hypothesis of Newton was again printed in the *L. E. and D. Philosophical Magazine* (Volume XXIX.), and it subsequently appeared in the Appendix to the first volume of Brewster's *Memoirs of Newton*, in 1855.

The time has come for farther inquiries into the science of Newton, and I shall endeavour to show that a careful examination of the writings of our great Natural Philosopher in the light of

[1] *L. E. and D. Philos. Magazine* [3] XXVIII. 106 and 478; also XXIX. 185.

the scientific progress of the last generation, renders still more evident the wonderful prevision of him who already, two centuries since, had anticipated most of the recent speculations and conclusions regarding cosmic chemistry.

As an introduction to the inquiries before us, and in order to show the real significance of the speculations of Newton, it will be necessary to review, somewhat at length, the history of certain views enunciated almost simultaneously by the late Sir Benjamin Brodie, of Oxford, and the present writer, and subsequently developed and extended by the latter. In part I. of his Calculus of Chemical Operations, read before the Royal Society, May 3, 1866, and published in the *Philosophical Transactions* for that year, Brodie was led to assume the existence of certain ideal elements. These, he said "though now revealed to us through the numerical properties of chemical equations only as *implicit and dependent existences,* we cannot but surmise may sometimes become, or may in the past have been, *isolated and independent existences.*" Shortly after this publication, in the spring of 1867, I spent several days in Paris with the late Henri Sainte-Claire Deville, repeating with him some of his remarkable experiments in chemical dissociation, the theory of which we then discussed in its relations to Faye's solar hypothesis. From Paris, in the month of May, I went, as the guest of Brodie, for a few days to Oxford, where I read for the first time and discussed with him his essay on the Calculus of Chemical Operations, in which connection occurred the very natural suggestion that his ideal elements might perhaps be liberated in solar fires, and thus be made evident to the spectroscope. I was then about to give, by invitation, a lecture before the Royal Institution on The Chemistry of the Primeval Earth, which was delivered May 31, 1867. A stenographic report of the lecture, revised by the author, was published in the *Chemical News* of June 21, 1867, and in the *Proceedings of the Royal Institution.* Therein I considered the chemistry of nebulae, sun and stars in the combined light of spectroscopic analysis and Deville's researches on dissociation, and concluded with the generalization that the "breaking up of compounds or dissociation of elements by intense heat is a principle of universal application, so that we may suppose that all the elements which make up the sun or our planet would, when so intensely heated as to be in the gaseous condition which all matter is capable of assuming, remain uncombined ;—that is to say would exist together in the state of chemical elements, whose farther dissociation in stellar or nebulous masses may even give us evidence of matter still more elemental than that revealed in the experiments of the laboratory, where we can only conjecture the compound nature of many of the so-called elementary substances."

The importance of this conception, in view of subsequent dis-

coveries in spectroscopy and in stellar chemistry, has been well set forth by Lockyer in his late lectures on Solar Physics [1], where however the generalization is described as having been first made by Brodie in 1867. A similar but later enunciation of the same idea by Clerk-Maxwell is also cited by Lockyer. Brodie, in fact, on the 6th of June, one week after my own lecture, gave a lecture on Ideal Chemistry before the Chemical Society of London, published in the *Chemical News* of June 14th, in which, with regard to his ideal elements, in farther extension of the suggestion already put forth by him in the extract above given from his paper of May 6, 1866, he says "we may conceive that in remote ages the temperature of matter was much higher than it is now, and that these other things [the ideal elements] existed in the state of perfect gases—separate existences—uncombined." He farther suggested, from spectroscopic evidence, that it is probable that "we may one day, from this source have revealed to us independent evidence of the existence of these ideal elements in the sun and stars."

During the months of June and July, 1867, I was absent on the continent, and this lecture of Brodie's remained wholly unknown to me until its republication in 1880, in a separate form by its author[2], with a preface, in which he pointed out that he had therein suggested the probable liberation of his ideal elements in the sun, referring at the same time to his paper of 1866, from which we have already quoted the only expression bearing on the possible independence of these ideal elements somewhere in time or in space.

The above statements are necessary in order to explain why it is that I have made no reference to Sir Benjamin Brodie on the several occasions on which, in the interval between 1867 and the present time, I have reiterated and enforced my views on the great significance of the hypothesis of celestial dissociation as giving rise to forms of matter more elemental than any known to us in terrestrial chemistry. The conception, as at first enunciated in somewhat different forms alike by Brodie and myself, was one to which we were both naturally, one might say, inevitably led by different paths from our respective fields of speculation, and which each might accept as in the highest degree probable, and make, as it were, his own. I write therefore in no spirit of invidious rivalry with my honoured and lamented friend, but simply to clear myself from the charge, which might otherwise be brought against me, of having on various occasions within the past fourteen years, put forth and enlarged upon this conception without mentioning Sir

[1] *Nature*, Aug. 25, 1881, Vol. xxiv. p. 396.

[2] *Ideal Chemistry*, a Lecture. Macmillan, 1880.

Benjamin Brodie, whose only publication on the subject, so far as I am aware, was his lecture of 1867, unknown to me until its reprint in 1880.

It was at the grave of Priestley, in 1874, that I for the second time considered the doctrine of celestial dissociation, commencing with an account of the hypothesis put forward by F. W. Clarke of Cincinnati, in January, 1873[1], to explain the growing complexity which is observed when we compare the spectra of the white, yellow and red stars; in which he saw evidence of a progressive evolution of chemical species by a stoichiogenic process from more elemental forms of matter. I then referred to the farther development of this view by Lockyer in his communication to the French Academy of Sciences in November of the same year, wherein he connected the successive appearance in celestial bodies of chemical species of higher and higher vapour-densities with the speculations of Dumas (and Pettenkofer) as to the composite nature of the chemical elements[2]. I then quoted from my lecture of 1867 the language already cited to the effect that dissociation by intense heat in stellar worlds might give us more elemental forms of matter than any known on earth, and farther suggested that the green line in the spectrum of the solar corona, which had been supposed to indicate a hitherto unknown substance, may be due to a "more elemental form of matter, which, though not seen in the nebulae, is liberated by the intense heat of the solar sphere, and may possibly correspond to the primary matter conjectured by Dumas, having an equivalent weight one fourth that of hydrogen." The suggestion of Lavoisier that "hydrogen, nitrogen and oxygen, with heat and light, might be regarded as simpler forms of matter from which all others are derived" was also noticed in connection with the fact that the nebulae, which we conceive to be condensing into suns and planets, have hitherto shown evidences only of the presence of the first two of these elements, which, as is well known, make up a large part of the gaseous envelope of our planet, in the forms of air and aqueous vapour. With this I connected the hypothesis that our atmosphere and ocean are but portions of the universal medium which, in an attenuated form, fills the interstellary spaces; and farther suggested "as a legitimate and plausible speculation," that "these same nebulae and their resulting worlds may be evolved by a process of chemical condensation from this universal atmosphere, to which they would sustain a relation somewhat analogous to that of clouds and rain to the aqueous vapour around us[3]."

[1] Clarke, "Evolution and the Spectroscope," *Popular Science Monthly*. New York, Vol. II. p. 32.

[2] Lockyer, *Comptes Rendus*, Nov. 3, 1873.

[3] *A Century's Progress in Theoretical Chemistry*, being an address at Northum-

These views were reiterated in the preface to a second edition of my *Chemical and Geological Essays* in 1878, and again before the British Association for the Advancement of Science at Dublin[1], and before the French Academy of Sciences in the same year[2]. They were still further developed in an essay on the Chemical and Geological Relations of the Atmosphere, published in the *American Journal of Science* for May 1880, in which attention was called to the important contribution to the subject by Mr Lockyer in his ingenious and beautiful spectroscopic studies, the results of which are embodied in his "Discussion of the Working Hypothesis that the so-called Elements are Compound Bodies," communicated to the Royal Society, December 12, 1878. It was then remarked that the already noticed "speculation of Lavoisier is really an anticipation of that view to which spectroscopic study has led the chemists of to-day;" while it was said that the hypothesis put forth by the writer in 1874, "which seeks for a source of the nebulous matter itself, is perhaps a legitimate extension of the nebular hypothesis."

To shew the connection of the above views with the philosophy of Newton, it now becomes necessary to give some account of the conception of the universal distribution of matter throughout space, both as regards its dynamical relations and its chemical composition. Passing over the speculations of the Greek physiologists, we come to the controversies on this subject in the seventeenth century, and find, in apparent opposition to the plenum maintained by Descartes and his followers, the teaching of Newton that "the heavens are void of all sensible matter." This statement is however qualified elsewhere by his assertion, that "to make way for the regular and lasting movements of the planets and comets it is necessary to empty the heavens of all matter, except perhaps some very thin vapours, steams and effluvia arising from the atmospheres of the earth, planets and comets, and from such an exceedingly rare etherial medium as we have elsewhere described," etc. (*Optics*, Book III. Query 28).

In order to understand fully the views of Newton on this subject it is necessary to compare carefully his various utterances, including the Hypothesis, in 1675, the first edition of the *Principia*, in 1687, the second edition, in 1713, and the various editions of the *Optics*. This work appeared in 1704, the third book, with its appended queries, having, according to its author's preface, been "put together out of scattered papers" subsequent to the publication of the first edition of the *Principia*. The Latin translation of

berland, Penn., July 31, 1874; *Amer. Chemist*. Vol. v. pp. 46—61, and *Pop. Science Monthly*, vi. p. 420.

[1] *Nature*, Aug. 29, 1878, Vol. XVIII. p. 475.

[2] *Comptes Rendus*, Sept. 23, 1878; Vol. XXXVIII. p. 452.

the *Optics*, by Dr Clarke, which was published in 1706, and the second English edition, in 1718, contain successive additions to these queries, which are indicated in the notes to Horsley's edition of the works of Newton, and are important in this connection. From a collation of all these, we learn how the conceptions of the Hypothesis took shape, were re-inforced, and in great part incorporated in the *Principia*.

In the Hypothesis he imagines "an etherial medium much of the same constitution with air, but far rarer, subtler, and more elastic." "But it is not to be supposed that this medium is one uniform matter, but composed partly of the main phlegmatic body of ether, partly of other various etherial spirits, much after the manner that air is compounded of the phlegmatic body of air, intermixed with various vapours and exhalations." Newton farther suggests in his Hypothesis that this complex spirit or ether, which, by its elasticity, is extended throughout all space, is in continual movement and interchange. "For nature is a perpetual circulatory worker, generating fluids out of solids, and solids out of fluids, fixed things out of volatile, and volatile out of fixed, subtile out of gross, and gross out of subtile; some things to ascend and make the upper terrestrial juices, rivers, and the atmosphere, and by consequence others to descend for a requital to the former. And as the earth, so perhaps may the sun imbibe this spirit copiously, to conserve his shining and keep the planets from receding farther from him; and they that will may also suppose that this spirit affords or carries with it thither the solary fuel and material principle of life, and that the vast etherial spaces between us and the stars are for a sufficient repository for this food of the sun and planets."

The language of this last sentence, in which his late biographer, Sir David Brewster, regards Newton as "amusing himself with the extravagance of his speculations," at which "we may be allowed to smile[1]," was not apparently regarded as unreasonable by its author when, more than ten years later, he quotes it in the postscript of his letter to Halley, dated Cambridge, June 20, 1686. The views therein contained, with the single exception of the suggestion regarding gravitation, have not wanted advocates in our own time, and many of them were embodied in the *Principia*, which Newton was then engaged in writing.

But this was not all: Newton saw in the cosmic circulation and the mutual convertibility of rare and dense forms of matter a universal law, and rising to a still bolder conception, which completes his Hypothesis of the Universe, adds: "Perhaps the whole frame of nature may be nothing but various contextures of some certain etherial spirits or vapours, condensed, as it were, by precipitation,

[1] Brewster's *Memoirs of Newton*, Vol. I. pp. 121 and 404.

much after the same manner that vapours are condensed into water, or exhalations into grosser substances, though not so easily condensible; and after condensation wrought into various forms, at first by the immediate hand of the Creator, and ever since by the power of nature, which, by virtue of the command 'increase and multiply,' became a complete imitator of the copy set her by the great Protoplast. Thus, perhaps may all things be originated from ether."

If now we look to the third book of the *Principia*, we shall find in proposition 41 the remarkable chemical argument by which Newton was led to regard the interstellary ether as affording "the material principle of life" and "the food of planets." Considering the exhalations from the tails of comets, he supposes that the vapours thus derived, being rarified, dilated, and spread through the whole heavens, are by gravity brought within the atmospheres of the planets, where they serve for the support of vegetable life. Inasmuch moreover as all vegetation is supported by fluids, and subsequently by decay is, in part, changed into solids, by which the mass of the earth is augmented, he concludes that if these essential matters were not supplied from some external source, they must continually decrease, and at last fail. This vital and subtile part of our atmosphere, so important, though small in amount, he then supposed might come from the tails of comets[1].

This appeared in the first edition of the *Principia*, in 1687. It was not until later that the conception of exhalations from other celestial bodies took shape in the mind of Newton, as we can learn from the *Optics*. Thus, in the first edition of this work, in Query 11, the sun and fixed stars are spoken of as great earths, intensely heated, and surrounded with dense atmospheres which, by their weight, condense the exhalations arising from these hot bodies. To this Query is added in 1706 the suggestion that the

[1] "Vapor enim in spatiis illis liberrimis perpetuò rarescit, ac dilatatur. Quâ ratione fit ut cauda omnis ad extremitatem superiorem latior sit quàm juxta capita cometae. Eâ autem rarefactione vaporem perpetuò dilatatum diffundi tandem, et spargi par coelos universos, deinde paulatim in planetas per gravitatem suam attrahi et cum eorum atmosphaeris misceri, ratione consentaneum videtur. Nam quemadmodum maria ad constitutionem Terrae hujus omnino requiruntur, idque ut ex iis per calorem Solis vapores copiosè satis excitentur, qui vel in nubes coacti decidant in pluviis, et Terram omnem ad procreationem vegetabilium irrigent et nutriant; vel in frigidis montium verticibus condensati (ut aliqui cum ratione philosophantur) decurrant in fontes et flumina: sic ad conservationem marium et humorum in planetis requiri videntur cometae ex quorum exhalationibus et vaporibus condensatis, quicquid liquoris per vegetationem et putrefactionem consumitur, et in Terram aridam convertitur, continuò suppleri et refici possit. Nam vegetabilia omnia ex liquoribus omninò crescunt, dein magnâ ex parte in Terram aridam per putrefactionem abeunt, et limus ex liquoribus putrefactis perpetuò decidit. Hinc moles Terrae aridae indies augetur, et liquores, nisi aliunde augmentum sumerent, perpetuò decresere deberent, ac tandem deficere. Porro suspicor spiritum illum, qui aëris nostri pars minima est, sed subtillissima et optima, et ad rerum omnium vitam requiritur, ex cometis praecipue venire."—NEWTON, *Principia*, lib. III. prop. XLI.

weight of such an atmosphere "may hinder the globe of the sun from being diminished except by the emission of light;" while in the second English edition, in 1718, we find a farther addition in the words, "and a very small quantity of vapours and exhalations." A similar change of view appears in the Query now numbered 28, we read of "places [almost] destitute of matter," and also that "the sun and planets gravitate towards each other without [dense] matter between." In these quotations, the two words in brackets are wanting in the edition of 1704, and first appear in that of 1718; while the language which we have in a previous page quoted from this same Query was added in the edition of 1706.

The Queries now numbered 17—24, appeared for the first time in the edition of 1718, and herein we find, in 18, the etherial medium spoken of as being "by its elastic force expanded through all the heavens." Of this medium "which fills all space adequately" he asks, "may not its resistance be so small as to be inconsiderable," and scarcely to make any sensible alteration in the movements of the planets[1]? This complex ether of the interstellary space was thus in the opinion of Newton made up in part of matter common to the planetary and stellar atmospheres, the origin and importance of which is concisely stated in the paragraph which appears for the first time in 1713, in the second edition of the *Principia,* in the third book, at the end of proposition 42, here much augmented. In this statement, which serves to supplement and complete that already made in 1687, in proposition 41, we read, that the vapours which arise alike from the sun, the fixed stars and the tails of comets, may by gravity fall into the atmospheres of the planets, and there be condensed, and pass into the forms of salts, sulphurs, (*id est,* combustible matters,) tinctures, clay, sand, coral, and other terrestrial substances[2].

The conception of Newton who, while rejecting alike the plenum of the Cartesians with its vortices, and an absolute vacuum, imagined space to be filled with an exceedingly attenuated matter, through which a free circulation of gaseous substances might take place between distant worlds, has found favour among modern thinkers, who seem to have been ignorant of his views. Sir William Grove in 1842 suggested that the medium of light and heat may be "a universally diffused matter," and subsequently, in 1843, in the chapter on Light, in his *Essay on the Correlation of*

[1] Compare this with Prop. x. Book III. of the *Principia.*

[2] "Vapores autem, qui ex Sole et stellis fixis et caudis cometarum oriuntur, incidere possunt per gravitatem suam in atmosphaeras planetarum, et ibi condensari et converti in aquam et spiritos humidos, et subinde per lentem calorem in sales, et sulphura, et tincturas, et limum, et lutum, et argillam, et arenam, et lapides, et coralla, et substantias alias terrestres paulatim migrare."—NEWTON, *Principia,* lib. III. prop. XLII.

Physical Forces, concluded with regard to the atmospheres of the sun and planets that there is no reason "why these atmospheres should not be, with reference to each other, in a state of equilibrium. Ether, which term we may apply to the highly attenuated matter existing in the interplanetary spaces, being an expansion of some or all of these atmospheres, or of the more volatile portions of them, would thus furnish matter for the transmission of the modes of motion which we call light, heat, etc.; and possibly minute portions of the atmospheres may, by gradual accretions and subtractions, pass from planet to planet, forming a link of material communication between the distant monads of the universe." Subsequently, in his address as President of the British Association for the Advancement of Science, in 1866, Grove farther suggested that this diffused matter may become a source of solar heat, "inasmuch as the sun may condense gaseous matter as it travels in space, and so heat may be produced."

Humboldt, also, in his *Cosmos*, considers the existence of a resisting medium in space, and says "of this impeding etherial and cosmical matter," it may be supposed that it is in motion, that it gravitates, notwithstanding its great tenuity, that it is condensed in the vicinity of the great mass of the sun, and that it may include exhalations from comets; in which connection he quotes from the 42nd proposition of the third book of the *Principia*. He farther speaks comprehensively of "the vaporous matter of the incommensurable regions of space, whether, scattered without definite limits, it exists as a cosmical ether, or is condensed in nebulous masses and becomes comprised among the agglomerated bodies of the universe[1]." Humboldt also cites in this connection a suggestion made by Arago in the *Annuaire du Bureau des Longitudes* for 1842, as to the possibility of determining, by a comparison of its refractive power with that of terrestrial gases, the density of "the extremely rare matter occupying the regions of space[2]."

In 1854, Sir William Thomson published his Note on the Possible Density of the Luminiferous Ether[3], wherein he remarks "that there must be a medium of material communication throughout space to the remotest visible body, is a fundamental conception of the undulatory theory of light. Whether or no this medium is (as appears to me most probable) a continuation of our own atmosphere, its existence cannot be questioned." He then attempts to fix an inferior limit to the density of the luminiferous medium in interplanetary space by considering the mechanical value of sun-

[1] *Cosmos.* Otté's translation, Harper's Ed., Vol. I. pp. 82, 86.

[2] *Ibid.* Vol. III. p. 40.

[3] *Trans. Roy. Soc. Edinburgh*, Vol. XXI. part 1; and *Philos. Mag.* 1855, Vol. IX. p. 36.

light, as deduced from the value of solar radiation and the mechanical equivalent of the thermal unit. He concludes "that the luminiferous medium is enormously denser than the continuation of the terrestrial atmosphere would be in interplanetary space if rarified according to Boyle's law always, and if the earth were at rest in a state of constant temperature, with an atmosphere of the actual density at its surface." The earth itself in moving through space "cannot displace less than 250 pounds of matter."

In 1870 W. Mattieu Williams published his very ingenious work entitled *The Fuel of the Sun*, in which, apparently without any knowledge of what had been written before with regard to an interstellary medium, he attempts to find therein the source of solar heat, the "solary fuel" of Newton. To quote his own language, "the gaseous ocean in which we are immersed is but a portion of the infinite atmosphere that fills the whole solidity of space, that links together all the elements of the universe, and diffuses among them light and heat, and all the other physical and vital forces which heat and light are capable of generating" (*loc. cit.* p. 5).

Since the days of Newton, however, no one had hitherto considered the interstellary matter from a chemical point of view. In 1874, as already shown, the writer had, in extension of the conception of Humboldt that its condensation gives rise to nebulae, ventured the suggestion that from an etherial medium having the same composition as our own atmosphere the chemical elements of the sun and the planets have been evolved, in accordance with the views of Brodie, Clarke, and Lockyer, by a stoichiogenic process, so that in the language of Newton's Hypothesis, "all things may be originated from ether."

It was not, however, until 1878, that, from a consideration of the chemical processes which have gone on at the earth's surface within recorded geological time, I was led to another step in this inquiry. That all the de-oxydized carbon found in the earth's crust in the forms of coal and graphite, as well as that existing in a diffused state as bituminous or carbonaceous matter, has come, through vegetation, from atmospheric carbonic acid, appears certain. To the same source we must ascribe the carbonic acid of all the limestones which since the dawn of life on our earth have been deposited from its waters. It is through the sub-aërial decay of crystalline silicated rocks, and the direct formation of carbonate of lime, or of carbonates of magnesia and alkalies, which have reacted on the calcium-salts of the primeval ocean, that all limestones and dolomites have been generated. These, apart from the coaly matter, hold, locked up and withdrawn from the aërial circulation, an amount of carbonic acid which may be probably estimated at not less than 200 atmospheres equal in weight to our own. That this

amount, or even a thousandth part of it, could have existed at any one time in our terrestrial atmosphere since the beginning of life on our planet is inconceivable, and that it could be supplied from the earth's interior is an hypothesis equally untenable.

I was therefore led to admit for it an extra-terrestrial source, and to maintain that the carbonic acid has thence gradually come into our atmosphere to supply the deficiencies created by chemical processes at the earth's surface. Since similar processes are even now removing from our atmosphere this indispensable element, and fixing it in solid forms, it follows that except volcanic agency, which can only restore a portion of what was primarily derived from the atmosphere, there are on earth, besides organic decay, only the artificial processes of human industry which can furnish carbonic acid; so that but for a supply of this gas from the interstellary spaces now, as in the past, vegetation, and consequently animal life itself, would fail and perish from the earth for want of this "food of planets."

Such were the conclusions, based on an induction from the facts of modern chemistry and geology, which I enunciated in my paper in 1878 and 1880, already quoted in the first part of this essay. I was at that time unacquainted with the Hypothesis of Newton, and with his remarkable reasoning contained in the 41st proposition of the third book of the *Principia*, in which he, so far as was possible with the chemical knowledge of his time, anticipated my own argument, and showed how and in what manner the interstellary ether may really afford the "food of planets," and, in a sense, "the material principle of life."

I have thus endeavoured to bring before the Philosophical Society of Cambridge, a brief history of the development of this conception of an interstellary medium, and to show that the thought of two centuries has done little more than confirm the almost forgotten views of Newton. It is with feelings of peculiar gratification that I have been able to indite these pages within the very walls of the College in which our great philosopher lived and laboured, and where, combining all the science of his time with a foresight which seems well-nigh divine, he was enabled, in the words of the poet, "to think again the great thought of the Creation."

(2) *On the Upper Bagshot Sands of Hordwell Cliffs, Hampshire.* By E. B. TAWNEY, M.A.

These sands appear in Hordwell Cliffs—at that part locally designated Long Mead End—and extend to a little W. of Beacon Bunny, where they run out at the top of the cliff. By old observers they were sometimes termed the "Estuarine-bed of Long Mead End" to distinguish them from the Marine Barton Clays below, and the more freshwater Lower Headon beds above. They have been recognised by most observers—from their position—as identical with similar sands in the Isle of Wight which come immediately below the Lower Headon beds. We will first refer to some of the older authorities who have treated of the district.

1814. T. WEBSTER. On the Freshwater Formations of the Isle of Wight, with some observations on the Strata over the Chalk in the South-east part of England. *Trans. Geol. Soc.*, 1st Ser. II. p. 161, pl. 9—11. In the map of the district all the "strata above the chalk" are coloured with one tint. In his very excellent coloured section of the coast between Headon Hill and Alum Bay in the Isle of Wight, these Upper Bagshot sands are indicated by a capital letter D. No details are given concerning them [p. 184] in the letter-press.

1822. A. SEDGWICK. On the Geology of the Isle of Wight. *Annals of Phil.*, New Series III. p. 329. Prof. Sedgwick having apparently lost his specimens and notes writes from memory, aided by memoranda made by his companion, Dr Whewell. On pp. 344, 348 he mentions that these "sandy strata contain a few marine shells, among which we remarked some very large Cerithia." He recognises the general parallelism of the Barton and Hordwell Cliff section to that of White Cliff Bay, though owing to the Barton clay at this time being universally confused with the London clay, the account of some of the beds is a little puzzling. The Lower Headon (freshwater) beds above the sands are recognised.

1824. T. WEBSTER. On a Freshwater Formation in Hordwell Cliff, Hampshire, and on the subjacent beds from Hordwell to Muddiford. *Trans. Geol. Soc.*, 2nd Ser. I. p. 90. After describing the freshwater Lower Headon [p. 93] we read "immediately below this formation at Hordwell is found a bed of sand from 60 to 100 feet thick, which appears first about Long Mead End, and may be well observed in the section at Beacon Bunny." The thickness is exaggerated, and the passage following as well as his section [pl. XII. fig. 1] shows that he has taken into it some of the Barton sands which are really separated by the Beacon Bunny clay, &c. The equivalence of the Hordwell "lower freshwater formation" [L. Headon] to that of the Isle of Wight is deduced from their both lying on this bed of sand, in which in some places intervenes a bed with a mixture of marine and freshwater shells.

1829. C. Lyell. On the Freshwater Strata of Hordwell Cliff, Beacon Cliff, and Barton Cliff, Hampshire. *Trans. Geol. Soc.*, 2nd Ser. II. p. 287, pl. XXX. A detail description is given of the freshwater Hordwell beds [L. Headon], and a coloured section [pl. XXX. f. 2] of the coast shows their disposition in the cliffs. The sands in question are described [p. 290] as No. 9; "white siliceous sand, without shells, rises near Long Mead End and extends through Beacon and Barton Cliffs to the middle of High Cliff." This is a repetition of the error in Webster's section. He misses their fossil contents apparently. Classifying them in the lower freshwater, he makes them extend too far to the west. They do not extend to Barton, much less to "about the middle of High Cliff." He further is in error in stating that "no portion of the upper marine formation [M. Headon] exists anywhere in this part of the Hampshire coast."

1838. D'Archiac. Note sur les sables et grès tertiaires. *Bull. Soc. Geol. Fr.*, S. I. t. IX. p. 54. After a description of the French beds of Barton age notices shortly these sands at Hordwell, and attributes a marine origin to them rather than freshwater.

1839. D'Archiac. Essais sur la coordination des terrains tertiaires du nord de la France, de la Belgique et de l'Angleterre. *Bull. Soc. Geol. Fr.*, Ser. I. t. X. p. 169. Mentions the lower freshwater at Hordwell reposing on these sands, which are correlated with the "sables moyens" [pp. 172, 200, 208].

1846. S. Wood. On the discovery of an Alligator and of several new Mammalia in the Hordwell Cliff, with observations upon the Geological Phenomena of that Locality. *Charlesworth's Geol. Journ.* I. p. 1. It is here shown that Lyell must have missed the upper marine [M. Headon], for it was recognised as a bed 10 inches think, and its fossils were collected by himself and Mr Edwards. He next corrects Lyell's view of the extent of the freshwater formation towards Barton. Our Upper Bagshot sand is described [p. 4] as coming below the freshwater deposit, and characterised as a "greyish-white or light-coloured "sand, containing the following molluscous genera, some species being "extremely abundant: *Oliva*, *Potamides*, *Ancillaria*, *Natica*, *Melania*, "*Melanopsis*, *Pleurotoma*, *Bulla*, *Mactra*, *Cyrena*, *Corbula*, *Sanguinolaria* "[Psammobia?], *Venericardia*, *Cytherea*, *Lucina*, *Potamomya*. The bed "appears to be intermediate between the London [Barton] clay and "the lower freshwater, and must be referred to an estuary formation, "for the largest proportion of its Testacea are referable to marine genera." He then proposes to term it the "Lower Marine" in opposition to the "Upper Marine," but the inadmissibility of such a term is apparent.

1846. J. Prestwich. On the Tertiary or Supracretaceous Formations of the Isle of Wight as exhibited in the Sections at Alum Bay and White Cliff Bay. *Q. J. G. S.* II. p. 223. We read, p. 227, "At Hordwell Cliff, on the top of the London [Barton] clay, there is a bed of sand fifteen feet thick overlaid by a few inches of grey clay." In the section [pl. IX. f. 1] from Alum Bay to Heatherwood Point, Isle of

Wight, these sands are termed Headon Hill sands, and estimated [p. 228] at 120 feet. At White Cliff Bay these sands are said to be 195 feet thick [No. 20 of pl. IX. f. 2, pp. 228, 243, 253]. They are supposed to be without organic remains, though he remarks that Prof. Sedgwick had found marine shells in the equivalent bed at Hordwell. The succeeding passage mentioning marine and estuary fossils at Hempstead refers of course to beds far higher in the series, and now classed as Miocene.

1846. S. Wood. On the Fossils and Geological Phenomena of the Hordwell Cliff. *Charlesworth's Geol. Journ.* I. p. 117. Merely mentioned the sands again as containing 27 genera of marine Mollusca.

1852. A. Dumont. Observations sur la Constitution Géologique des terrains tertiaires de l'Angleterre comparés à ceux de la Belgique. *Bull. Ac. R. Belg.* XIX. In the table at end of the essay the Headon Hill sands are grouped with the Barton clay as equivalent respectively to the upper and lower divisions of the Belgian Laekenien, while the Headon Hill limestones and marls are placed in the Tongrien.

1852. E. Hébert. Comparison des couches tertiaires inférieures de la France et de l'Angleterre. *Bull. Soc. Geol. Fr.*, Ser. 2, t. IX. p. 350. In a table in which a parallel is drawn between the deposits of the Hampshire, Thames and Paris basins, these sands are correlated with the unfossiliferous sands of Monneville and of the Mortefontaine heath [that is to say the upper part of the sables de Beauchamp].

1852. Marchioness of Hastings. Description géologique des falaises d'Hordle, sur la côte du Hampshire, en Angleterre. *Bull. Soc. Geol. Fr.*, Ser. 2, t. ix. p. 191. A measured section of Hordwell freshwater beds with their fossils. Bed 17, about 20 feet thick, is the sand in question. It is described as sand of a variable colour, greyish-green where it rises from below the sea-level, but becoming lighter westward, so that beyond Long Mead End it is pale yellow with ferruginous bands and grey spots, p. 202. The first five feet are said to be full of shells but not disposed in lines; but the thickness over which fossils extend diminishes, so that after a time fossils disappear. Remains of *Lamna*, *Myliobates*, *Ætobates* are mentioned; but the Crocodile, *Trionyx*, *Potamomya*, come from the bed in contact with it above, and which is the bottom bed of the freshwater formation.

1852.? T. Wright. A Stratigraphical Account of the Section from Round Tower Point to Alum Bay on the N. W. coast of the Isle of Wight. *Proc. Cotsw. Club*, I. p. 87, and *Ann. Mag. Nat. Hist.*, Ser. 2, VII. p. 14. These sands in the Isle of Wight, at Headon Hill, are mentioned as containing "fragments of shells too minute and waterworn to ascertain to what genus they belonged" [p. 98]. They are said [p. 99] to be probably of estuary origin. The equivalent bed at Beacon Cliff containing estuary and marine genera, together with the bones of turtles and teeth of sharks.

1852. T. Wright. A Stratigraphical Account of the Section of Hordwell, Beacon and Barton Cliffs, on the coast of Hampshire. *Proc. Cotsw. Club*, I. p. 120. The Hordwell freshwater and "upper marine"

[M. Headon] is described bed by bed. The sands in question are said [p. 128] to rise about 300 yards W. of Long Mead End and run out at Beacon Bunny, inclined at an angle of 2°. They are divided into No. 16, greyish-white sand scarcely coherent, the fossils not well preserved; the 14 genera cited have been previously mentioned by S. Wood: one specific name *Lucina divaricata* is mentioned. Thickness of the grey sand 5 feet, and it passes gradually into No. 17, of which it may be considered to form the upper fossiliferous portion. No. 17, fine white sand, non-fossiliferous; estimated at 15 to 20 feet. Much importance is attached to the fossil contents from the light which they throw on the conditions under which the [equivalent] Headon Hill sands were deposited, being probably of estuary origin. [We may add that the upper freshwater [U. Headon], as described by Dr Wright, above the "upper marine" does not exist at Hordle; probably some of the L. Headon, the position of which has been mistaken, are referred to.]

1853. E. Forbes. On the Fluvio-marine Tertiaries of the Isle of Wight. *Q. J. G. S.* IX. p. 259. In this important paper, which first announced the distinctness of the Hempstead [Hamstead] series from the Headon Hill beds, and confirmed the identity of the Colwell Bay and Headon Hill marine beds, there is a mention, p. 268, of the sands below the "lower beds of the Headon series. No fossils have hitherto been re-"corded from these beds or the Isle of Wight. At White Cliff Bay how-"ever, to all appearance barren, they are highly fossiliferous, containing "abundant impressions of marine shells, apparently of Barton species. "The shelly matter has entirely disappeared...the specimens are quite "untransportable."

1856. E. Forbes. On the Tertiary Fluvio-marine Formation of the Isle of Wight. *Memoirs of the Geological Survey of Great Britain.* This Survey Memoir, giving the details on which the conclusions of the preceding paper were founded, contains the following passage, p. 86: "The "lower beds of the Headon series rest upon sands known as Headon Hill "sands, which are the equivalents of the Upper Bagshot beds...these "sands rest upon the Barton clays or highest portion of the Middle "Bagshot group."

1856. J. Prestwich. On the Correlation of the Eocene Tertiaries of England, France and Belgium, Part II. *Q. J. G. S.* XIII. p. 89. At p. 108 we read, "In the Barton series I would now include the "siliceous sands at the base of Headon Hill and at the top of Barton "Cliff. For though they contain no organic remains at those spots, casts "of marine shells, apparently of the same species as those in the under-"lying clay occur in considerable numbers in parts of the sands occupying "the same position at White Cliff Bay; whilst at Barton, as the deposit "ranges eastward towards Hordwell Cliff, these sands alternate with fos-"siliferous grey clays, and form with them passage beds between the "purely marine and compact Barton clay and the freshwater beds nearer "Milford." p. 109, "At the Barton section a thin bed of sand, full of "estuarine shells, is interposed between the Barton sands and the fresh-"water clays." [L. Headon.]

1859. H. W. Bristow. Explanation of Sections across the Isle of Wight, illustrative of Geological Map, Sheet No. 10. In this explanation, and in the legend to the Survey Vertical Sections, the above facts collected by the Survey are repeated.

1862. H. W. Bristow. The Geology of the Isle of Wight. *Mem. Geol. Surv.*, Sheet 10, pp. 50—52. A fuller account of the Survey results than given in Forbes' Memoir or Appendix to it. The casts of bivalves found in these sands at White Cliff Bay are said to be *Cardium, Tellina, Panopæa.*

1864. A. Von Koenen. On the Correlation of the Oligocene deposits of Belgium, Northern Germany and the South of England. *Q. J. G. S.* xx. p. 97. In a note to p. 99 this sand at Headon Hill is classed with the Barton beds, because at Hordwell it contains many characteristic Barton shells, *Oliva Branderi*, &c.

From the excellent descriptions of the late Searles Wood and Marchioness of Hastings, it will be seen that the sands in question are from fifteen to twenty feet in thickness. Our experience agrees too with former descriptions, that the fossils are confined to the upper part of the sands, where they are so crowded together that sometimes the shells contribute more to the substance of the bed than the sand. The *Cyrena* and *Melania* are the most abundant, after them perhaps comes the *Cerithium pleurotomoides.*

The object of the present communication is to discuss the affinities of the deposit with a view to the classification, and also to glance at its equivalent in the Paris basin.

With regard to the actual position of the sand below the freshwater Lower Headon all observers are agreed, and its more marine character in contrast with these was insisted on by Searles Wood.

We are confronted however with less unanimity when we consider the question of its classification in the Tertiary system—whether it should be placed in the Middle or Upper Eocene, &c.

There are two views to be noticed. Even so early as 1838 D'Archiac was inclined to classify it with the marine [Barton] beds below, rather than with the freshwater deposits above.

Forbes and the Geological Survey most distinctly ally it to the marine Bagshot beds; they place it in the Middle Eocene Bagshot series, terming it Upper Bagshot (instead of Headon Hill sands, the designation of some previous authors) sands. Forbes noticing its containing apparently Barton species at White Cliff Bay presents to us thus its affinity to Barton beds. Dumont[1] may be said to have favoured a similar classification.

An opposite view is that lately advanced by Prof. Judd[2]; in discussing the development of the Headon Hill beds, he writes [p. 169],

[1] *Loc cit.* pp. 37, 48.

[2] On the Oligocene Strata of the Hampshire Basin. *Q. J. G. S.* xxxvi. (1880), p. 137.

"These Headon Hill sands are usually called the Upper Bagshot "beds; but it appears to me that it cannot be but a source of con-"fusion to base our classification of the Upper Eocene strata on the "poorly fossiliferous deposits of the London basin rather than on "the richly fossiliferous deposits of the Hampshire basin. It is "true that at Alum Bay the Headon Hill sands have not yielded "any fossils, but the equivalent beds at Hordwell contain a by no "means scanty fauna, in which we find the same admixture of "marine and freshwater forms which characterises the overlying "Headon strata. As moreover we detect in these beds the emi-"nently characteristic *Cerithium concavum*, it seems clear that we "must regard them as constituting the lowest member of the "Headon group." Accordingly in the table comparing the English beds with foreign equivalents [*l. c.* p. 167] the whole of these Upper Bagshot sands, Lower and M. Headon beds are paralleled with the Mortefontaine sands, which are placed *above* the St Ouen freshwater limestone. [This error is avoided by the omission of the St Ouen limestone in a similar table, *Pop. Sc. Rev.* IV. New Ser. 1880, p. 133.]

Since *Oliva Branderi*, Sow., occurs abundantly in these sands, a shell characteristic of the Barton beds and not passing up into the M. Headon marine beds, it has seemed to us that the view just cited was not to be accepted without further evidence.

Accordingly we made a journey to France to examine the Mortefontaine bed, collect its fossils for comparison with those from the Long Mead End sands, and trace its connection with the St Ouen limestone, and beds above, as well as with the "Sables de Beauchamp" below,—which latter is equivalent to our Barton deposits.

If Searles Wood had been able to determine the species in the numerous genera which he collected from this bed, doubtless the true affinities of the bed would have been recognised, and Forbes' opinion would scarcely have been challenged. The result of our collecting from the Hordwell sands is given in a table, in which we have also noted in separate columns such of the species as pass down into Barton beds, or upwards into Middle Headon marine deposits, as well as those recognised in the Mortefontaine sands of the Paris basin—here deemed equivalent.

We have compared our specimens with those in the Edwards collection in the British Museum, which were determined by Edwards himself; in cases where we have not accepted his names we have in an appendix given reasons for so doing.

The result of our work is that out of a total of 28 species which we collected from the Long Mead End sands, 15 occur in the Barton beds below, while 10 pass up into the Middle Headon[1].

[1] This is including the Brockenhurst bed at the base of the M. Headon, where many Barton species cease without extending up into the Venus-bed.

After eliminating those which occur both in Barton and M. Headon beds, the per centages become; common to Barton beds and not to M. Headon 43·4 per cent.; common to M. Headon and not to Barton 21·3 per cent. As far as fossil evidence is concerned therefore, these sands are more related to the Barton beds than they are to the Headon.

We have next to deal with the argument that they must be classed with the Headon series because both contain *C. concavum*, Sow. It is true they contain a shell which has hittherto been confounded with that species, but probably falsely so. I made a point of collecting some hundred of this shell, and have little doubt now that it is *C. pleurotomoides*, Lam.: we wish to correct therefore our former commingling of these species[1] in which we followed previous authors. The object of examining the equivalent beds in France was to see whether the distribution of this species there was such an extended one as would have been the case if it ranged from the Long Mead End sands to the top of the M. Headon. I am much indebted to Prof. Hébert and M. Munier-Chalmas, the eminent authorities at the Sorbonne, for pointing out to me that the Mortefontaine sands do not contain *C. concavum* at all, and that the shell so common at that horizon is *C. pleurotomoides*, Lam., which they deem distinct. The French sands near Mortefontaine preserve the shells in most perfect condition, so that there is no doubt about the identification there. Unfortunately at Long Mead End, the shells are so corroded and the ornaments so poorly preserved that the distinctness from the M. Headon shell has not been hitherto recognised. On comparing it however on the one hand with examples brought from near Mortefontaine, and on the other hand with the M. Headon shell, it becomes evident that the Long Mead End species agrees with the former only[2]. It differs from *C. concavum* not only in the tubercles when preserved, but also in the curve of the mouth aperture, and form of the notch or sinus there.

Owing to the courtesy of M. Munier-Chalmas, I have been made acquainted with results which he is about to publish, and which point to a much greater parallelism between the French and English series than has hitherto been acknowledged. The Mortefontaine sands are merely the upper part of the Sables de

[1] *Q. J. G. S.* XXXVII. p. 107.

[2] So long as the Cerithium of both horizons, viz. the Long Mead End sands and Middle Headon was determined as identical, in that sense *C. concavum* had a wide range in England. We think it, however, due to a misconception to state as has been done [*Q. J. G. S.* XXXVII. p. 126] that all foreign geologists regard the zone of this species as the top of the Barton. They have written only of the Long Mead End or Mortefontaine sands as such. We may trust, I think, foreign geologists to know the relations between the Mortefontaine sands and the St Ouen limestone.

Beauchamp representing our Barton beds: above this comes the Calcaire de St Ouen, mainly freshwater. Connected with St Ouen limestone are sands and marls, with abundance of *C. concavum*, Sow., a layer near the base, and another at the top. It is plain then that the St Ouen period represents our Headon series, only that our beds are thicker. Moreover, in the Hampshire basin the fresh-water and marine conditions in the Headon series are not in the same order as in the St Ouen beds. With us the marine facies with *C. concavum* comes between the two freshwater Lower and Upper Headon deposits, respectively 100 and 50 feet thick. Near Montjavoult M. Munier-Chalmas represents the bulk of the fresh-water limestone, in the centre between two deposits with this Cerithium. In France therefore the *C. concavum* zone is not connected with the *C. pleurotomoides* zone but comes distinctly above it. The Mortefontaine sand with *C. pleurotomoides* is classed by French authorities as the upper part of the "Sables de Beauchamp" or Barton beds. The fossils of the Paris basin Tertiaries are usually so much better preserved than in equivalent English beds, and the faunas have been so much better worked out in France—partly no doubt from this circumstance, and partly from the different zones being found frequently in different localities—that we may derive great assistance in classifying our beds from comparison with the classical series of the Paris basin.

In the Table following an attempt is made to correlate the beds on either side of the Channel. The thickness of the English beds is given as a guide to those who have not actually visited them, and we hope these details may aid in comparison with foreign equivalents[1]. In the French series some of the minor subdivisions are omitted. The line between Miocene and U. Eocene[2] is drawn according to Lyell, a method of classification adopted

[1] As an instance of misconceptions which it is desirable to clear up may be mentioned the following; Prof. Sandberger [*Suesswasser Conchylien*, p. 312 note] citing *Melania turritissima* as occurring at Hordwell and Hamstead, intimates that these beds are of the same horizon. It will be seen by the Table that about 250 feet of beds intervene.

[2] Though we do not wish to enter into the question here of the utility of the term Oligocene, we may remark that we consider the fine sections of the Isle of Wight show that it is less applicable to the English Tertiaries than the older and more classical division into Eocene, &c. This is shown too by the fact that foreign authorities who have attempted to apply this term in the correlation of English with German beds are not agreed about the boundaries. Thus Von Koenen makes the M. Headon, including the Brockenhurst bed, the equivalent of the German L. Oligocene. But Prof. C. F. Sandberger (*Land und Suesswasser Conchylien der Vorwelt*, p. 198) makes the St Ouen limestone or Cerithium concavum zone the top of the Upper Eocene, so that the Lower Oligocene boundary is shifted higher up, and our M. Headon is Upper Eocene in this scheme. In other words the break between Oligocene and Eocene is an unnatural one, and the introduction of the term in our opinion obscures the affinities between the members of the English series.

TABLE COMPARING PART OF THE HAMPSHIRE WITH PARIS BASIN TERTIARIES.

			THICKNESS.	
L. MIOCENE	1.	Hamstead, marine *Ostr. cyathula*, *Voluta Rathieri*, *C. plicatum*	15 ft.	1. Calcaire de Beauce 2. Sables de Fontainebleau *Ostr. cyathula*, *Voluta Rathieri*, *C. plicatum*
	2.	Hamstead marls, freshwater *Cyr. semistriata*, *Bithinia Chastelii*	170 ft.	3. Calcaire de Brie, *Bithinia Chastelii* 4. Marnes vertes
		Bembridge marls, freshwater with *Cyr. obtusa*, *C. semistriata* and a marine band with *Ostrea vectensis*	70—90 ft.	5. Marnes jaunes, *Cyrena semistriata* (*convexa*)
UPPER EOCENE		Bembridge limestone *Anoplotherium commune*, *Palæoth. magnum*	25 ft.	1. Gypse, *Anoplotherium commune*, *Palæoth. magnum* 2. Marnes à *Pholadomya ludensis*
MIDDLE EOCENE	1.	Osborne marls and limestone, freshwater	80 ft.	
	2.	Upper Headon, ditto *Lymnæa longiscata* (HEADON SERIES)	50 ft.	Calcaire de St Ouen *Lymnæa longiscata*
	3.	Middle Headon, marine *Murex sexdentatus*, *Pisania labiata* *Ostr. velata* including the Brockenhurst bed (HEADON SERIES)	33—90 ft.	
	4.	Lower Headon clays, sands, and limestone, freshwater (HEADON SERIES)	85—100 ft.	
	5.	Upper Bagshot sands, *C. pleurotomoides*	15—180 ft.	Sables et calcaire de Mortefontaine, *C. pleurotomoides* Calcaire de Ducy
	6.	Barton beds (M. Bagshot) *N. Prestwichiana*	300 ft.	Sables de Beauchamp, *Nummulina variolaria*
	7.	Braklesham beds (M. Bagshot) *N. lævigata* *Card. planicosta*	300 ft.	Calcaire grossier, *Numm. lævigata*
	8.	Lower Bagshot sands, unfossiliferous	660 ft.	Sables de Cuise-la-Motte, *Numm. planulata*

from French authorities. The Geological Survey is followed as far as the Middle Eocene is concerned.

The parallelism of the beds in both basins is conspicuous when arranged in this way. It will be noticed in passing that in the Hampshire basin the beds placed opposite the Calcaire de St Ouen are very much thicker than that deposit, while on the other hand the thickness of the French gypsum series is enormous compared to our Bembridge limestone. It is possible therefore that our Osborne freshwater beds which succeed the *C. concavum* zone are contemporaneous with the lower part of the gypsum series[1].

The analogy between the Long Mead End sands and those of Mortefontaine concerns us more. Thát they are equivalents few can doubt on considering that both immediately succeed the Barton beds, or rather constitute its uppermost portion; the fossils which they have in common—about 25 per cent.—also tends to confirm that conclusion; while if the *Cerithium* which each contains is acknowledged as identical, the proof is still more positive.

From this would follow, that the Upper Bagshot sands is the most fitting name to express the relationship of these sands, since the Barton and Bracklesham beds together are usually considered as the equivalents of the M. Bagshot. Of course few would be disposed to maintain that the division—Upper Bagshot sand—is exactly equivalent in the Hampshire and Bagshot areas. Fossils being scarce in the latter no detail horizons can be drawn there. It would be hard to say what part of the Middle Bagshot correspond to the top of the Barton beds in Barton cliffs. Indeed some considerations have lately been put forward[2] which make it probable the Upper Bagshot in Surrey partly represent the Barton beds, since the fossils which occur in them are Barton forms. Prof. Prestwich[3] had previously suggested the same thing.

We think that we have shown how inadvisable it would be to

[1] Perhaps the most minute comparison between English and Foreign Tertiaries is that made by Prof. C. Mayer, *Tableau Synchronistique des Terrains Tertiaires inférieures.* Zurich, 1869. 4th edit. Here we find the Mortefontaine beds divided into two divisions, viz. fossiliferous above, which are paralleled with the Long Mead End sands, and unfossiliferous below, placed on a line with the calcaire of St Ouen and unfossiliferous sands of Mortefontaine. We prefer to consider that the unfossiliferous sands of the Senlis and Mortefontaine district are simply part of the Barton sands, and they are separated from the St Ouen limestone by the fossiliferous Mortefontaine bed proper, where developed. There are some misconceptions in this table arising from insufficient distinction between beds in the English localities. Thus the lacustrine [L. Headon] of Hordle seems mixed up with the Upper Headon, Osborne, and Bembridge limestone of Headon Hill. This throws the whole correlation into confusion. The affinity of the Brockenhurst bed is thereby misunderstood. We have endeavoured to rectify this in the table now put forward.

[2] W. H. Herries, On the Bagshot Beds of the Bagshot district, *Geog. Mag.* XVIII. p. 172.

[3] *Q. J. G. S.* XIII. p. 132.

reject Forbes' name of Upper Bagshot for the Long Mead End sands, in favour of the older term of Headon Hill sands.

Besides the ready information of kind friends at the Faculté des Sciences, I have to acknowledge assistance obtained from Mr H. Keeping, whose intimate knowledge of the Hampshire Tertiaries has been freely put at my disposal.

FOSSILS FROM THE LONG MEAD END SANDS, COLLECTED 1880-1.

	M. Headon beds.	Long Mead End Sand.	Barton or Bracklesham beds.	Mortefontaine bed.
Otodus obliquus, Aq.		×	×	
Lamna elegans, Aq.		×	×	
Myliobates toleapicus, Aq.		×	×	
Marginella simplex, Edw.	?	×		
Pisania larata, Sow.		×	×	
Fusus (young, indeterminable)		×		
Oliva Branderi, Sow.		×	×	
Ancillaria buccinoides, Lam.	×	×	×	
Natica abscondita, Desh.		×		×
Cerithium pleurotomoides, Lam.		×		×
—— pyrgotum, Ed. MS.		×		
—— speculatum Ed. MS.		×		?
—— deperditum, Desh.		×		×
Melania hordacea, Lam.		×	×	×
Melanopsis fusiformis, Sow.		×		
Calyptræa trochiformis, Lam.	×	×	×	×
Ringicula ringens, Lam.		×	×	×
Bulla Lamarckii, Desh.	×	×		
Nucula tumescens, Edw.		×	×	
Lucina gibbosula, Lam.		×		?
Strigilla colvellensis, Ed. MS.	×	×		
Cyrena deperdita, Desh.		×	×	×
Cardita oblonga, Sow.	×	×	×	
Cytherea suessonensis, Desh. = Cyth. tenuistria, Sow.		×	×	
Mactra filosa, Ed. MS.	?	×		
Psammobia endis, Lam.	×	×	×	?
Corbula Edwardsii, n. sp.	×	×		
—— Cuspidata, Sow.	×	×	×	
Total	8	28	15	7

Notes on some of the Fossils.

Marginella simplex, Edw. This is cited only from Long Mead End in Edwards' *Eocene Mollusca*, Univalves, p. 144, but in his collection in the British Museum is a specimen labelled as from Colwell Bay; from an examination of the matrix in which it was imbedded we think this latter also from Long Mead End.

Pisania lavata, Sow. A variety with a secondary thread between the larger spiral ones in addition to still finer ones; it occurs in the Barton beds also with the type.

Natica abscondita, Desh. One specimen agrees exactly with French specimens which I collected in the equivalent bed near Mortefontaine, Paris basin: the shell is punctate. I cannot find any punctations in *N. Studeri*, this is the readiest means of distinction; our shell is also shorter and broader relatively, and the spire more inclined to be turriculate from increased flattening of the younger whorls at the suture.

N. labellata occurs in the Beacon Bunny bed below and in the Middle Headon above, but we do not happen to have found it in the intermediate W. Bagshot sand.

Cerithium pleurotomoides, Lam. This shell has with us hitherto been determined as *C. concavum*, Sow., and considered identical with it. It was the identification of this latter shell with Lamarck's species which led some observers to correlate the Middle Headon with the fossiliferous zone of Mortefontaine, or uppermost part of the "Sables de Beauchamp." However, M. Munier-Chalmas has discovered the distinctness of these two species, and the fact of their occupying different horizons. By comparison of Long Mead End specimens with French examples from near Mortefontaine we recognise their identity, and so establish a perfect correspondence between the French and English series. Thus *C. concavum* characterises the M. Headon of Headon Hill, Colwell Bay, and Hordwell, while *C. pleurotomoides* is found only in the Upper Bagshot sand of Long Mead End.

The most ready way of distinguishing these species is the curve of the aperture or sinus of the outer lip, which curve also persists in the growth lines which mark successive apertures: in *C. concavum* the sinus is deeper so that the lines above and below it meet at an angle of rather less than 90°, while in *C. pleurotomoides* the angle is considerably over 90° or even 100°. The ornaments in Long Mead End examples are rarely well enough preserved to make them available for ready distinction.

C. pyrgotum, Ed. MS. We have several examples of a shell so determined by Edwards in his collection, in some points however it is more like *C. mutabile*, Lam., a Bracklesham shell than

Edwards' Lower Headon species. The type of Edwards' species occurs in the Lower Headon, but we have not seen it from the M. Headon.

C. speculatum, Ed. MS. This name is founded on a single imperfect specimen in the Edwards collection; it seems to us probably identical with *C. tricarinatum*, Lam., which occurs in the Mortefontaine beds; if so it would be another connecting link between that horizon in France and our Upper Bagshot sands.

C. deperditum, Lam. Edwards identified a few specimens in his collection from the Long Mead End bed as Lamark's species; we have collected one also this year, and refer it to this species which occurs in the Mortefontaine beds.

C. cavatum, Ed. MS. A single example from the Long Mead End bed in the Edwards collection has this appellation given to it, but it is evidently only a smooth variety of the *C. pleurotomoides*, Lam.

[*C. margaritaceum*, var. *recentius*. One specimen so labelled by Edwards is in his collection as from Long Mead End; it comes from a green sandy bed, but evidently not from the Upper Bagshot.]

Melania hordacea, Lam. A small Melania is very common in the Long Mead End bed; Edwards has determined it in his collection as *M. fasciata*, Sow. We consider it however nearer to Lamark's species, and can find no point in which it differs therefrom. It is entirely without the cross ridges or tubercles which ornament the young whorls of *M. fasciata*. Moreover the aperture in this species is much narrower than in the Long Mead End shell, and the whorls are rather flatter, having even a slight depression near the suture in the last whorl corresponding to the base of the tubercular ridge in the young whorls. Even when the shells are corroded as they usually are in the Long Mead End bed, the form of the aperture is distinctive.

M. fusiformis, Sow. Not uncommon in the Long Mead End bed, passes up through Lower to M. Headon.

Bulla Lamarkii, Desh. Two examples identical with ours are so identified by Edwards in his collection. This species has not been found yet above the lower beds (Brockenhurst beds) of the M. Headon.

Nucula tumescens, Edw. In Wood's *Eocene Bivalves*, p. 121, said to be from Long Mead End and Barton on Edwards' authority; this Barton locality is the Beacon Bunny bed usually considered the top bed of the Barton; we have collected it ourselves from this bed. It is not known as yet further down in the Barton, neither does it pass up into the Headon beds.

[*Lucina inflata*, Ed. MS. A shell is so determined by Edwards in his collection from Long Mead End and Barton, is quite different from individuals from the Headon beds of Headon Hill,

and in fact we find it so closely allied to *L. gibbosula*, Lam., that we scarcely can separate them; the French shell may be a little flatter.]

L. gibbosula, Lam., is also recognised by Edwards as existing in the Long Mead End bed; it is a rather less wide example than the preceding. We cite both varieties under Lamarck's name for the present.

[*L. pratensis*, Ed. MS., is founded on a single imperfect specimen from Long Mead End in the Edwards collection; it may possibly be a fragment of the preceding. It is better not to cite so uncertain a species.]

Strigilla Colvellensis, Edw. MS. This species we found abundant in the Long Mead End bed; in the Edwards collection are also 32 from this bed and 4 from M. Headon localities. In form it is much like *S. Rigaultiana*, Desh., of the Barton beds, but differs in the angle at which the divaricating ornaments diverge; in the latter it is about 90°, but in *S. Colvellensis* the lines form an angle considerably over 100°; this is the readiest way of distinguishing the species, the hinge is much alike in both: *S. Rigaultiana* is slightly thicker in texture.

The hinge has in left valve two diverging cardinal teeth leaving a triangular pit between them, the right valve has one cardinal and two distant lateral teeth which fit into depressions of the other valve.

Cyrena deperdita, Desh, is determined by Prof. Morris [Appendix to *Isle of Wight Survey Memoir*, p. 156] from the Long Mead End bed. A shell is very abundant at Long Mead End which we can scarcely separate from Deshayes' species brought from near Mortefontaine; we identify it therefore provisionally. It was however determined by Edwards as *C. gibbosula*, Morr., though under the same term were included other forms from the M. Headon bed of Hordwell. The *C. deperdita* in Edwards' collection from the Barton beds is a wider and flatter shell rather.

C. gibbosula, Morr. Under this term Morris (*ibidem*, pl. VI., fig. 13, 13 *a*) figures perhaps two different forms; it is described as from "Headon Series"—which part of the series is not indicated. In Edwards' collection it comes from Lower Headon, Beacon Bunny clay, and Long Mead End sands.

[*C. pisum*, Desh.? In the Edwards collection under this name are included shells which seem a shorter variety only of *C. gibbosula*, Morr. (Edw.), they come from the Long Mead End sands and Beacon Bunny bed below.]

[*C. cycladiformis*, Desh.?, so determined by Edwards from Long Mead End and Beacon Bunny beds, seems a flatter form of his *C. gibbosula*. We do not consider it identical with the French type, and as we have even doubts as to its separate specific

existence, it is not cited in our list. The type occurs in much lower beds in France.]

[*C. arenaria*, Forbes, labelled in the Edwards collection as from Long Mead End, is not from the U. B. sands. This shell from the Lower Headon seems frequently determined as *C. deperdita;* we fell into the error ourselves; it is a higher and flatter shell.]

Mactra filosa, Edw. MS. We have one specimen which we consider the same as the unique example from the Long Mead End sands on which Edwards founds his name; it is a little more trigonal than *M. fastigiata*, Ed. MS., but until more specimens are found its title to specific rank is uncertain. Some dwarfed poorly preserved specimens in the Edwards collection are labelled as from Colwell Bay, but from the character of the matrix they perhaps come from Long Mead End.

[*M. fastigiata* occurs in the Beacon Bunny clay.]

Cytherea Suessonensis, Desh. = *C. tenuistria*, Sow. This shell, of which we have 4 examples from the Long Mead End sands, is determined in the Edwards collection as *C. suborbicularis*, Ed. MS., but it is evidently of the same species as that which we collected in the Beacon Bunny clay below, and which Edwards has determined as *C. tenuistria*, Sow. This is a variable shell if we adopt the limits allowed to it in his collection; we have here to do with the variety wider in antero-posterior direction. It differs from *C. suborbicularis*, Ed. MS., in being a little more inflated, the lunule more impressed makes the outline there a little straighter, the umbos a little more turned inwards, the lower margin less curved, the growth striæ coarser, and the ligament was less exposed.

Corbula Edwardsii, n. sp. A small corbula is abundant in the Long Mead End sands, which Edwards in his collection has determined as *C. nitida*, Sow.; he has it also from Roydon. We remark that Sowerby (about 1823 A.D.) describing *C. nitida*, says it was "first observed by Prof. Sedgwick in several parts of the Isle of Wight below the upper freshwater formation." At that time the Hamstead and Headon Hill beds were not separated, so that the stratigraphical position is not trustworthy. The probability therefore is that the shells were collected at Hamstead, and are the *C. Vectensis*, Forbes; this is almost certain on comparing Sowerby's figures, *Min. Conch.* IV. p. 85, pl. 162, figs. 1—3, they agree entirely with the Hamstead species. Prof. Morris in editing the Appendix to Forbes' *Memoir on Isle of Wight*, p. 145, suggests that the new name was a synonym of Sowerby's. The Woodwardian Museum possess specimens collected by Sedgwick about 1820 at Hamstead, and they have always borne the name of *C. nitida*, Sow. Though there is no record of their being the

identical individuals lent to Sowerby for figuring, it is quite plain that they would be the same species. Hence *C. Vectensis*, Morris, must rank as a synonym of *C. nitida*, Sow., which was collected at Hamstead.

Deshayes in 1824 gave the name *C. nitida* to a shell from the Calcaire grossier [*Coq. foss. Env. Paris*, I. p. 57, pl. 8, figs. 39—41], but as he removed it subsequently to the genus *Sphenia* there is no question of ambiguity.

Having to give a new name to the Long Mead End species, it may well be called after our most diligent explorer of Eocene Mollusca.

The valves are unequal, right valve considerably overlapping the other, both the posterior and low borders. The left or smaller valve is slightly angulated with a posterior area, but the feature is more obscure in the other valve. The shell is rather smooth, but with delicate growth-striæ. It is very much flatter and smaller than *C. striata*; the outline is not unlike that of *C. oblonga*, Desh., but it is less inequilateral; the umbos are nearly median. Length $\frac{1}{4}$ inch, by rather less than $\frac{3}{16}$ breadth.

Corbula cuspidata, Sow. We have this both from the Long Mead Mead End sands and the Beacon Bunny clay below, it occurs lower down in the Barton and above in the Headon beds.

[*C. fortisulcata*, Edw. MS. There is a shell so determined in the Edwards collection as from Long Mead End; it is however from clay, and not from the Upper Bagshot sands: it is perhaps only a variety of *C. pisum*, Sow.]

[*Chama squamosa*, Brand. It is said on the authority of Dr T. Wright, in Lyell's *Student's Elements of Geology*, p. 233, that this shell occurs in the Upper Bagshot sands. This is apparently an error; the specimen must have come from Barton sandy beds.]

(3) *On some equations connected with the Electromagnetic Theory of Light.* By R. T. Glazebrook, M.A.

The problem of the reflexion and refraction of Light on Maxwell's Theory has been treated by Lorentz (*Schlömilch Zeitschrift*, Vol. XXII.), and again by J. J. Thomson, (*Phil. Mag.*, April, 1880), so far at least as isotropic media are concerned, and also by Fitzgerald (*Phil. Trans.* 1881). The following paper gives a method of obtaining the intensities of the reflected and refracted plane waves in the most general case, that of a wave travelling in a crystalline medium incident on the surface of another crystal, which is more simple than those of Lorentz or

Fitzgerald. Some of the results are expressed in form capable of direct experimental verification.

We shall suppose that the dielectric medium remains at rest and that the coefficient of magnetic induction μ is the same in all directions. This assumption is made by Maxwell, *Electricity and Magnetism*, Vol. II. § 794.

Let (F, G, H), (a, b, c), (P, Q, R), be the components parallel to the axes of the vector-potential of electric induction, of magnetic induction, and of electromotive force in a medium whose coefficient of magnetic induction is μ, and whose specific inductive capacities parallel to the same three axes are K_1, K_2, and K_3 (the axes being principal axes).

Then Maxwell, Vol. II. § 598, we have the equations

$$P = -\frac{dF}{dt} - \frac{d\psi}{dx},$$

and two similar ones, also

$$a = \frac{dH}{dy} - \frac{dG}{dz},$$

etc.

Again if f, g, h are the displacements

$$f = \frac{K_1}{4\pi} P \qquad\qquad (1),$$

$$g = \frac{K_2}{4\pi} Q \qquad\qquad (2),$$

$$h = \frac{K_3}{4\pi} R \qquad\qquad (3),$$

and

$$\frac{df}{dx} + \frac{dg}{dy} + \frac{dh}{dz} = 0.$$

The term ψ may be supposed to contain the E. M. F. due to the polarization of the dielectric.

Differentiate (1) with respect to y, (2) with respect to x. Divide by $K_1/4\pi$, $K_2/4\pi$, respectively, and subtract

$$4\pi \left\{\frac{1}{K_1}\frac{df}{dy} - \frac{1}{K_2}\frac{dg}{dx}\right\}$$

$$= \frac{dP}{dy} - \frac{dQ}{dx}$$

$$= \frac{dc}{dt} \qquad\qquad (4).$$

Similarly

$$4\pi\left\{\frac{1}{K_3}\frac{dh}{dx}-\frac{1}{K_1}\frac{df}{dz}\right\}=\frac{db}{dt}\quad\text{..................(5)},$$

$$4\pi\left\{\frac{1}{K_2}\frac{dg}{dz}-\frac{1}{K_3}\frac{dh}{dy}\right\}=\frac{da}{dt}\quad\text{..................(6)}.$$

If u, v, w are the components of the current in the dielectric medium

$$4\pi\mu u=\frac{dc}{dy}-\frac{db}{dz}\quad\text{........................(7)},$$

etc.,

$$u=\frac{df}{dt}.$$

Then

$$\frac{d^2f}{dt^2}=\frac{du}{dt}$$

$$=\frac{1}{4\pi\mu}\frac{d}{dt}\left\{\frac{dc}{dy}-\frac{db}{dz}\right\}$$

$$=\frac{1}{\mu K_1}\left\{\frac{d^2f}{dy^2}+\frac{d^2f}{dz^2}\right\}-\frac{1}{\mu K_2}\frac{d^2g}{dxdy}-\frac{1}{\mu K_3}\frac{d^2h}{dzdx}.$$

Put

$$\mu K_1=\frac{1}{\bar{a}^2},\ \text{etc.}$$

Then the general equations of electric displacement are

$$\frac{d^2f}{dt^2}=\bar{a}^2\left(\frac{d^2f}{dy^2}+\frac{d^2f}{dz^2}\right)-\bar{b}^2\frac{d^2g}{dxdy}-\bar{c}^2\frac{d^2h}{dzdx}\quad\text{.........(8)},$$

$$\frac{d^2g}{dt^2}=\bar{b}^2\left(\frac{d^2g}{dz^2}+\frac{d^2g}{dx^2}\right)-\bar{c}^2\frac{d^2h}{dydz}-\bar{a}^2\frac{d^2f}{dxdy}\quad\text{.........(9)},$$

$$\frac{d^2h}{dt^2}=\bar{c}^2\left(\frac{d^2h}{dx^2}+\frac{d^2h}{dy^2}\right)-\bar{a}^2\frac{d^2f}{dzdx}-\bar{b}^2\frac{d^2g}{dydz}\quad\text{......(10)}:$$

$\bar{a}$, $\bar{b}$, $\bar{c}$ are the principal velocities in the crystal and must not be confused with a, b, c the components of magnetic induction. Maxwell uses the same symbols for both.

Now suppose we are considering a plane wave whose equation at time t is $lx+my+nz-Vt=0$, and let λ, μ, ν be the direction cosines of f, g, h.

Put $lx + my + nz - Vt = \omega$ and let the displacement be S, some function of ω, so that

$$f = \lambda S,$$
$$g = \mu S,$$
$$h = \nu S.$$

Then

$$l\lambda + m\mu + n\nu = 0 \ldots\ldots\ldots\ldots\ldots\ldots\ldots (11),$$

or the disturbance is in the wave front, and

$$\lambda\{^2V - \bar{a}^2(m^2+n^2)\} + \mu\bar{b}^2lm + \nu\bar{c}^2nl = 0,$$
$$\lambda\bar{a}^2lm + \mu\{V^2 - \bar{b}^2(n^2+l^2)\} + V\bar{c}^2mn = 0,$$
$$\lambda\bar{a}^2nl + \mu\bar{b}^2mn + \nu\{V^2 - \bar{c}^2(l^2+m^2)\} = 0;$$

therefore $\quad \lambda(V^2 - \bar{a}^2) + l\{\bar{a}^2l\lambda + \bar{b}^2m\mu + \bar{c}^2n\nu\} = 0$, etc.;

therefore $\quad \dfrac{l}{\bar{a}^2 - V^2} = \dfrac{\lambda}{\bar{a}^2l\lambda + \bar{b}^2m\mu + \bar{c}^2n\nu}$, etc.

Multiply by l, m, n respectively, and add

$$\frac{l^2}{\bar{a}^2 - V^2} + \frac{m^2}{\bar{b}^2 - V^2} + \frac{n^2}{\bar{c}^2 - V^2}$$
$$= \frac{l\lambda + m\mu + n\nu}{\bar{a}^2l\lambda + \bar{b}^2m\mu + \bar{c}^2n\nu} = 0 \ldots\ldots\ldots\ldots\ldots\ldots (12).$$

Thus the relation between the direction of propagation and the velocity of the wave is that given by Fresnel.

Moreover, eliminating from the three equations the quantities V^2 and $\bar{a}^2l\lambda + \bar{b}^2m\mu + \bar{c}^2n\nu$, we get

$$\frac{l}{\lambda}(\bar{b}^2 - \bar{c}^2) + \frac{m}{\mu}(\bar{c}^2 - \bar{a}^2) + \frac{n}{\nu}(\bar{a}^2 - \bar{b}^2) = 0 \ldots\ldots\ldots (13).$$

Thus the direction of electric displacement is also given by Fresnel's construction.

These conclusions have already been arrived at by Maxwell, *Electricity and Magnetism*, Vol. II. § 798, but he has given the equations satisfied by F, G, H. In problems connected with the reflexion and refraction of the waves the conditions are expressed more easily in terms of f, g, h, and so it became necessary to form the equations these quantities satisfy.

We proceed next to the equations satisfied by the components of magnetic induction.

We have a, b, c, being these components:

$$\frac{da}{dt} = 4\pi\left\{\frac{1}{K_2}\frac{dg}{dz} - \frac{1}{K_3}\frac{dh}{dy}\right\},$$

therefore
$$\frac{d^2a}{dt^2} = 4\pi\left\{\frac{1}{K_2}\frac{dv}{dr} - \frac{1}{K_3}\frac{dw}{dy}\right\},$$
$$\text{for } v = \frac{dg}{dt}, \text{ etc.};$$

therefore
$$\frac{d^2a}{dt^2} = \frac{1}{\mu K_2}\frac{d}{dr}\left(\frac{da}{dz} - \frac{dc}{dx}\right) - \frac{1}{\mu K_3}\frac{d}{dy}\left(\frac{db}{dx} - \frac{da}{dy}\right).$$

Putting $\bar{a}$, $\bar{b}$, $\bar{c}$ for the principal coefficients,
$$\frac{d^2a}{dt^2} = \bar{b}^2\frac{d^2a}{dz^2} + \bar{c}^2\frac{d^2a}{dy^2} - \bar{b}^2\frac{d^2c}{dzdx} - \bar{c}^2\frac{d^2b}{dxdy},$$
and two similar equations. These equations have been given by J. J. Thomson for an isotropic medium in his paper already referred to. From these we can shew that the magnetic induction is propagated with a velocity given by the same construction while its direction also satisfies Fresnel's formula. It is interesting to notice that the equations for the magnetic induction and the electric displacement are different in form, though they lead both to Fresnel's construction for the velocity of propagation.

It is frequently necessary to determine the magnetic force in terms of the electric displacement.

If α, β, γ be the components of magnetic force,
$$\frac{d\alpha}{dt} = 4\pi\left\{\bar{b}^2\frac{dg}{dz} - \bar{c}^2\frac{dh}{dy}\right\}, \text{ etc.},$$
$$f = \lambda S, \text{ etc.},$$
S being a function of
$$lx + my + nz - Vt \equiv w,$$
and λ, μ, ν direction cosines of electric displacement,
$$\frac{dg}{dz} = n\frac{dS}{dw} = \frac{n}{V}\frac{dS}{dt},$$
$$\therefore \alpha = \frac{4\pi}{V}\left\{\bar{b}^2\mu n - \bar{c}^2\nu m\right\}S,$$
and two similar equations.

Multiply by l, m, n respectively, and add, the sum is zero.

Thus the direction of magnetic force is in the wave front.

Multiply by λ, μ, ν respectively, and add, then since
$$\frac{l}{\lambda}(\bar{b}^2 - \bar{c}^2) + \frac{m}{\mu}(\bar{c}^2 - \bar{a}^2) + \frac{n}{\nu}(\bar{a}^2 - \bar{b}^2) = 0,$$
the sum is again zero. Thus the direction of the magnetic force is perpendicular to the electric displacement.

We know moreover that the velocity of propagation is inversely proportional to the radius vector of the ellipsoid

$$a^2x^2 + b^2y^2 + c^2z^2 = 1,$$

(a, b, c being here the principal velocities, not the components of magnetic induction, with which we are not at present concerned,) which is parallel to λ, μ, ν.

Let p be the perpendicular on the tangent plane to this ellipsoid at λ, μ, ν; r the length of the radius vector. Then

$$\frac{1}{p^2} = (a^4\lambda^2 + b^4\mu^2 + c^4\nu^2)\, r^2.$$

Let χ be the angle between the perpendicular on the tangent and the radius vector $\qquad p = r \cos\chi$,

therefore $\qquad a^4\lambda^2 + b^4\mu^2 + c^4\nu^2 = \dfrac{1}{r^4\cos^2\chi} = V^2 \sec^2\chi.$

Again, we know that

$$\frac{l}{\lambda(a^2 - V^2)} = \frac{m}{\mu(b^2 - V^2)} = \frac{n}{\nu(c^2 - V^2)}$$

$$= \frac{1}{\sqrt{\{\lambda^2a^4 + \mu^2b^4 + \nu^2c^4 - 2V^2(a^2\lambda^2 + b^2\mu^2 + c^2\nu^2) + V^4\}}} = \frac{1}{V^2\tan\chi},$$

therefore $\qquad l = \dfrac{\lambda(a^2 - V^2)}{V^2\tan\chi}$, etc.,

therefore $\qquad \alpha = \dfrac{4\pi\mu\nu}{V}(c^2 - b^2)\cot\chi \,.\, S \qquad\qquad (15).$

Again, if λ', μ', ν' are the direction cosines of α, β, γ, we have

$$\lambda' = m\nu - n\mu$$

$$= \frac{\mu\nu\{b^2 - V^2 - (c^2 - V^2)\}}{V^2\tan\chi} = \frac{\mu\nu(b^2 - c^2)}{V^2\tan\chi},$$

therefore

Similarly,

$$\left.\begin{aligned} \alpha &= 4\pi V\lambda' S \\ \beta &= 4\pi V\mu' S \\ \gamma &= 4\pi V\nu' S \end{aligned}\right\} \qquad\qquad (16).$$

Thus the value of the magnetic force corresponding to an electric displacement S is $\qquad 4\pi \,.\, V \,.\, S$,
and its direction lies in the wave front and is perpendicular to the electric displacement.

We knew previously that this was the case for a plane wave in an isotropic medium. Thus the crystal produces the same effect

as an isotropic medium whose specific inductive capacity is equal to that of the crystal in the direction of the electric displacement.

An important distinction between the magnetic and electric displacements must be noted. The velocity of propagation of both is given by the same construction, that is to say, it is inversely proportional to one of the axes of the section of a certain ellipsoid by the wave front. The electric displacement is parallel to that axis which determines the velocity while the magnetic is perpendicular to it.

The electromotive force in the crystal we know is not in the direction of the electric displacement.

If we construct an ellipsoid whose axes are $\sqrt{K_1}$, $\sqrt{K_2}$, $\sqrt{K_3}$ and if r be the radius vector of the ellipsoid in the direction of displacement, and p the perpendicular on the tangent plane, then it is easy to shew that the E. M. F. is in the direction of this perpendicular and is equal to $4\pi S'/rp$, S' being the electric displacement.

Again, if χ be the angle between this perpendicular and the direction of displacement, and we resolve this force along S, and at right angles to it in a plane passing through r and p, we have for the components

$$\frac{4\pi S'}{rp}\cos\chi, \quad \text{and} \quad \frac{4\pi S'}{rp}\sin\chi,$$

but

$$p = r\cos\chi.$$

Thus the E. M. F. along and perpendicular to S' respectively, is

$$\frac{4\pi S'}{r^2}, \quad \text{and} \quad \frac{4\pi S'}{r^2}\tan\chi.$$

And if K' be the specific inductive capacity of the medium in the direction of displacement $r^2 = K'$ and the components are

$$\frac{4\pi S'}{K'}, \quad \text{and} \quad \frac{4\pi S'}{K'}\tan\chi.$$

If a wave of transverse displacement be traversing the crystal since the direction of displacement is an axis of the section of the wave surface by the plane of the wave it is clear that the direction of this last component is the wave normal. And the angle χ is, if the medium be magnetically isotropic, the angle between the ray and the wave normal. Thus the E. M. F. produced by a displacement S' in a direction in which K' is the specific inductive capacity is

$$\frac{4\pi S'}{K'} \text{ in direction of displacement,}$$

$$\text{and } \frac{4\pi S'}{K'} \tan\chi \text{ along the wave normal.}$$

The electrostatic energy per unit of volume is

$$\begin{aligned}
&\tfrac{1}{2}\{fP + gQ + hR\} \\
&= 2\pi\left\{\frac{f^2}{K_1} + \frac{g^2}{K_2} + \frac{h^2}{K_3}\right\} \\
&= 2\pi\mu\,\{a^2\lambda^2 + b^2\mu^2 + c^2\nu^2\}\,S^2 \\
&= 2\pi\mu V^2 S^2 \quad\text{..} \quad (17).
\end{aligned}$$

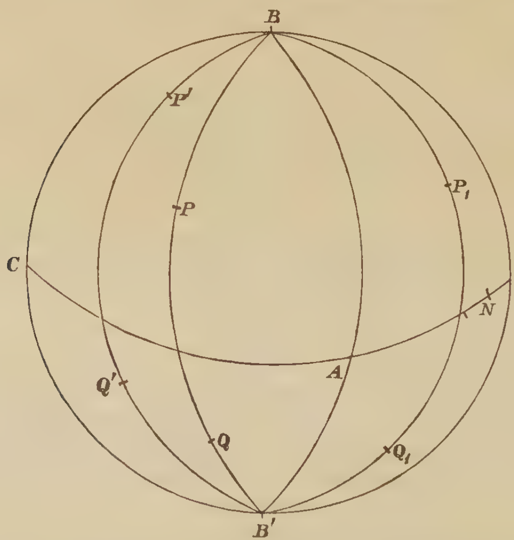
The electro-kinetic energy is

$$\begin{aligned}
&\frac{\mu}{8\pi}(\alpha^2 + \beta^2 + \gamma^2) \\
&= 2\pi\mu V^2 S^2 \quad\text{..} \quad (18),
\end{aligned}$$

And as before with isotropic media the two are equal.

We proceed now to consider the reflexion and refraction of a plane wave incident on a crystalline surface.

Take a unit sphere and let a plane parallel to the face of



incidence cut it in BCB'. The incident reflected and refracted waves cut the face of incidence in the same line.

Let BB' be parallel to this. Let BPB', BP_1B', and $BP'B'$ be the sections of the sphere by planes parallel to the incident, the reflected and the refracted waves, and let lines parallel to the directions of electric disturbance in these waves cut the sphere in P, P_1, P' and lines parallel to the directions of the magnetic disturbance in Q, Q_1Q', $PQ = P_1Q_1 = P'Q' = \frac{\pi}{2}$, and the directions PQ, etc. are related to those in which the wave travels as the directions of rotation and translation of a right-handed screw.

Let the normal to the surface drawn inwards meet the sphere in A and let $BC = \frac{\pi}{2}$.

The surface conditions are, (1) That the magnetic and electric displacements resolved normal to the surface should be the same in the two media. (2) That the electric and magnetic forces parallel to the surface should be the same.

Thus the magnetic and electric displacements in the direction A are to be the same. This gives two conditions, and the magnetic and electric forces parallel to B and C are to be the same. This gives four other conditions. Thus there are six conditions in all, which, as we shall see, reduce to four.

We treat the first medium as isotropic. Let K be the specific inductive capacity, V the velocity of light in the incident wave, ϕ the angle of incidence, ϕ_1 that of reflexion. We consider at first only one refracted wave. Let V' be the velocity of that wave, K' the specific inductive capacity in the direction in which the displacement in it takes place, χ the angle between the refracted ray and wave normal, ϕ' the angle of refraction. Also let θ, θ_1, θ' be the angles which the directions of vibration make with B, B'. Let S, S_1, S' be the electric displacements in the three waves.

The electromotive forces are

$$\frac{4\pi}{K}S, \qquad \frac{4\pi}{K}S_1$$

for the incident and reflected waves, and for the refracted,

$$\frac{4\pi}{K'}S'$$

along the direction of displacement,

and $$\frac{4\pi}{K'}S' \tan\chi$$

along the wave normal.

The magnetic forces are

$$4\pi VS, \qquad 4\pi V_1 S_1, \qquad 4\pi V'S',$$

and the magnetic displacements

$$4\pi\mu VS, \qquad 4\pi\mu V_1 S_1, \qquad 4\pi\mu' V'S'.$$

Take the electric conditions first.

Resolving, we have for electric displacements in direction A,

$$S \cos AP + S_1 \cos AP_1 = S' \cos AP' \dots\dots\dots\dots (18).$$

Electric forces in direction B,

$$\frac{S}{K} \cos BP + \frac{S_1}{K_1} \cos BP_1 = \frac{S'}{K'} \cos BP' \dots\dots\dots\dots (19).$$

(Since the wave normal is at right angles to BB' the electric force along the wave normal has no component in direction B.)

In direction C,

$$\frac{S}{K} \cos CP - \frac{S_1}{K_1} \cos C'P_1 = \frac{S'}{K'} \cos CP' + \frac{S'}{K'} \tan\chi \cos NC \dots (20).$$

Magnetic displacement in direction A,

$$\mu VS \cos AQ + \mu V_1 S_1 \cos AQ_1 = \mu' V'S' \cos AQ' \dots\dots (21).$$

Magnetic force in direction B,

$$VS \cos B'Q + V_1 S_1 \cos B'Q_1 = V'S' \cos B'Q' \dots\dots\dots (22).$$

In direction C,

$$VS \cos CQ - V_1 S_1 \cos C'Q_1 = V'S' \cos CQ' \dots\dots (23).$$

Also $$\frac{1}{K} = \mu V^2, \quad \frac{1}{K'} = \mu' V'^2,$$

$$\frac{V}{\sin\phi} = \frac{V'}{\sin\phi'} = \frac{V_1}{\sin\phi_1} = \rho, \text{ say},$$

$$\cos AP = \sin\theta \sin\phi = V \cos B'Q$$
$$= V \sin\theta = \rho \sin\theta \sin\phi.$$

Thus (18) and (22) reduce to

$$S \sin\theta \sin\phi + S_1 \sin\theta_1 \sin\phi_1 = S' \sin\theta' \sin\phi'.$$

Again $$\frac{1}{K} \cos BP = \mu V^2 \cos\theta = \mu\rho^2 \sin^2\phi \cos\theta,$$

$$\mu V \cos AQ = \mu V \cos\theta \sin\phi$$
$$= \mu\rho \sin^2 \cos\theta,$$

and (19) and (21) become

$$\mu S \sin^2 \phi \cos \theta + \mu_1 S_1 \sin^2 \phi_1 \cos \theta_1 = \mu' S' \sin^2 \phi' \cos \theta'.$$

Also
$$\frac{1}{K} \cos CP = \mu V^2 \sin \theta \cos \phi$$
$$= \mu\rho^2 \sin^2 \phi \sin \theta \cos \phi.$$

Thus (20) gives

$$\mu S \sin^2 \phi \sin \theta \cos \phi - \mu_1 S \sin^2 \phi \sin \theta_1 \cos \phi_1$$
$$= \mu' S' \sin^2 \phi' (\sin \theta' \cos \phi' + \tan \chi \sin \phi').$$

Again $\quad V \cos CQ = V \cos \theta \cos \phi = \rho \cos \theta \cos \phi \sin \phi.$

Thus (23) becomes

$$S \cos \theta \cos \phi \sin \phi - S_1 \cos \theta_1 \cos \phi_1 \sin \phi_1 = S' \cos \theta' \cos \phi' \sin \phi'.$$

We will re-write these four equations in two pairs, remembering that since the first medium is isotropic, $\phi = \phi_1$.

$$\left.\begin{array}{l}(S \cos \theta + S_1 \cos \theta_1)\, \mu \sin^2 \phi = S' \cos \theta' \mu' \sin^2 \phi' \ldots\ldots\ldots\ldots\ldots(24)\\ (S \cos \theta - S_1 \cos \theta_1) \cos \phi \sin \phi = S' \cos \theta' \cos \phi' \sin \phi' \ldots\ldots(25)\end{array}\right\}$$

$$\left.\begin{array}{l}(S \sin \theta + S_1 \sin \theta_1) \sin \phi = S' \sin \theta' \sin \phi' \ldots\ldots\ldots\ldots\ldots\ldots\ldots(26)\\ (S \sin \theta - S_1 \sin \theta_1)\, \mu \sin^2 \phi \cos \phi \\ \qquad = \mu' S' \sin^2 \phi' (\sin \theta' \cos \phi' + \tan \chi \sin \phi' \ldots\ldots(27)\end{array}\right\}.$$

If both media be isotropic we have here four unknowns, viz. S_1, S', θ_1, θ' and these equations are sufficient to determine them.

If the second medium be a crystal, θ' is not unknown, it has one of two definite values depending only on the position of the refracted wave with reference to the optic axes. Thus we have only three variables in our four equations and they require modification. Now we assumed that there was only one refracted wave. In general when light falls on a crystal there are two, we must therefore introduce into each of our four equations terms involving quantities S'', θ'', ϕ'', μ'' in the same way as θ', ϕ', μ' are involved, θ'' being a known function of ϕ''. Again if the first medium also be crystalline, θ_1 is not variable, and we are again reduced to three variables and four equations. Moreover terms involving the angle between the ray and the wave normal in the incident and reflected rays would be introduced into our fourth equation (27).

But we know that we have two reflected waves, and we must add to our equations terms involving S_2, θ_2, ϕ_2, μ_2 in the same manner as S_1, θ_1, ϕ_1, μ_1 are involved.

Thus the first of our equations of condition θ becomes in the case of a wave passing from one crystal to another

$$\mu S \cos\theta \sin^2\phi + \mu_1 S_1 \cos\theta_1 \sin^2\phi_1 + \mu_2 S_2 \cos\theta_2 \sin^2\phi_1 = \mu' S' \cos\theta' \sin^2\phi' + \mu'' S'' \cos\theta'' \sin^2\phi'',$$

while for the fourth we have

$$\begin{aligned} \mu s \sin^2\phi\, \{\sin\theta \cos\phi + \tan\chi \sin\phi\} \\ - \mu_1 S_1 \sin^2\phi_1\, \{\sin\theta_1 \cos\phi_1 + \tan\chi_1 \sin\phi_1\} \\ - \mu_2 S_2 \sin^2\phi_2\, \{\sin\theta_2 \cos\phi_2 + \tan\chi_2 \sin\phi_2\} \\ = \mu' S' \sin^2\phi'\, \{\sin\theta' \cos\phi' + \tan\chi' \sin\phi'\} \\ + \mu'' S'' \sin^2\phi''\{\sin\theta'' \cos\phi'' + \tan\chi'' \sin\phi''\}, \end{aligned}$$

χ, χ_1 etc. being the angles between the rays and wave normals in the respective waves. The other equations can be written down in a similar manner.

They may moreover be simplified by remembering that in all known transparent dielectrics μ differs but little from unity, we may therefore put very approximately

$$\mu = \mu_1 = \mu_2 = \mu' = \mu'',$$

but even then the equations are very complicated to solve.

Let us return to our four original equations (24—27), S, S_1, S' are here electrical displacements. If we require the amplitude of the light vibration we must remember that the total energy corresponding to a displacement S is $4\pi\mu V^2 S^2$. Now the amplitude of the light vibration varies as the square root of the energy, thus if σ, σ_1, σ' be the amplitudes required remembering that μ is the same for the two media

$$\sigma : \sigma_1 : \sigma' = VS : V_1 S_1 : V'S' = S \sin\phi : S_1 \sin\phi_1 : S' \sin\phi'.$$

The formulæ given above agree with those found by J. J. Thomson, *Phil. Mag.* April, 1880, if we put $\chi = 0$ in the fourth equation so that the second medium also is isotropic.

As has been said already they agree also with the results of Lorentz and Fitzgerald. It will be useful to give as Lorentz has done the relations between θ, θ' and θ_1 when only one wave traverses the crystal.

We easily find by eliminating the ratios S/S_1 and S/S' from the equations (24—27),

$$\tan\theta = \tan\theta' \cos(\phi - \phi') + \frac{\sin^2\phi' \tan\chi}{\cos\theta' \sin(\phi + \phi')} \quad \text{............(28)},$$

and

$$\tan\theta_1 = -\tan\theta\,\frac{\cos(\phi+\phi')}{\cos(\phi-\phi')} + \frac{2\sin 2\phi\,\sin^2\phi'\tan\chi}{\sin 2(\phi-\phi')\sin(\phi+\phi')\cos\theta'} \quad \ldots(29).$$

The first of these equations has been recently tested by me experimentally, when the second medium was a piece of Iceland spar. For angles of incidence between 40° and 55° the results of theory and experiment agreed fairly. For higher angles there were marked differences amounting to about 15 per cent. of the quantities measured, for angles below 40° differences also were found to exist. An abstract of the paper giving details of the experiments was read before the Royal Society on Nov. 17, 1881.

The equations (24—27) are the same as those given by the theories of Neumann, MacCullagh and Kirchhoff, to express the relations between θ, θ', and θ_1.

ISLAND OF JERSEY

The figures denote the distance from St Heliers

Cambridge University Press.

PROCEEDINGS

OF THE

Cambridge Philosophical Society.

February 6, 1882.

PROFESSOR BABINGTON, VICE-PRESIDENT, IN THE CHAIR.

Mr E. J. Love, St John's College, was balloted for and duly lected an Associate of the Society.

The following communications were made to the Society:

(1) *On the Composition of Albumen, and the changes which Leucine and similar bodies undergo in the animal system*, by P. W. LATHAM, M.D., Downing Professor of Medicine, Physician to Addenbrooke's Hospital.

The formula by which the composition of the proteids is enerally represented, viz. that of Lieberkühn*, is

$$C_{72}\ H_{112}\ N_{18}\ O_{22}\ S$$

Schützenberger†, who devoted some three years to the analysis of gg-albumen, gives as its composition

$$C_{240}\ H_{387}\ N_{65}\ O_{75}\ S_3$$

These two formulæ differ chiefly in the amounts of carbon and ulphur; for multiplying the first by $3\frac{1}{2}$ we get

$$C_{252}\ H_{392}\ N_{63}\ O_{77}\ S_{3\frac{1}{2}}$$

To resolve such a formula into one representing the proximate contituents of a proteid seems at the first glance hopeless. The ollowing considerations, however, give us some help in the solution f the problem:

1. Several of the products which occur in or can be obtained rom the animal organism, such as lactic acid, leucine, benzoic acid,

* Fownes, *Manual of Chemistry*, 1877, p. 625.
† *Annales de Chimie*, 1879, p. 384.

can also be obtained artificially from the cyanhydrins of the alcoho[ls] and ketones.

2. Lactic acid is produced when a muscle dies, or when it con[-]tracts, and by lactic fermentation from sugar. It can also be ob[-]tained

(i) by oxidising ethylic alcohol, treating the aldehyde [so] obtained with hydrocyanic acid to form a cyanhydrin, and actin[g] upon this with acids or alkalis—

$$\underset{\text{ethyl alcohol}}{C_2H_5.HO} + O = \underset{\text{aldehyde}}{CH_3.CHO} + H_2O$$

$$\underset{\text{aldehyde}}{CH_3.CHO} + CNH = \underset{\text{cyanhydrin}}{CH_3.CH\begin{cases}OH\\CN\end{cases}}$$

$$CH_3.CH\begin{cases}OH\\CN\end{cases} + 2H_2O = NH_3 + \underset{\text{ethidene lactic acid}}{CH_3.CH\begin{cases}OH\\COOH\end{cases}}\ *$$

(ii) By converting ethene alcohol or glycol into a cyanhydri[n] and boiling with acids or alkalis—

$$\underset{\text{glycol}}{\begin{matrix}CH_2.OH\\|\\CH_2.OH\end{matrix}} + HCl = H_2O + \underset{\text{chlorhydrin}}{CH_2\begin{cases}CH_2OH\\Cl\end{cases}}$$

$$CH_2\begin{cases}CH_2OH\\Cl\end{cases} + KCN = KCl + \underset{\text{cyanhydrin}}{CH_2\begin{cases}CH_2OH\\CN\end{cases}}$$

$$CH_2\begin{cases}CH_2OH\\CN\end{cases} + 2H_2O = \underset{\text{ethene lactic acid}}{CH_2\begin{cases}CH_2OH\\COOH\end{cases}} + NH_3\ \dagger$$

2. Leucine is very largely diffused in the animal organism, an[d] may be obtained by various processes from albumen, flesh, gelatin[,] casein, &c.‡

By oxidising amylic alcohol with potassium chromate and sulphu[-]ric acid, and distilling, we obtain amylic or valerianic aldehyde §—

$$\underset{\text{amylic alcohol}}{C_5H_{11}.HO} + O = \underset{\text{valerianic aldehyde}}{C_4H_9.CHO} + H_2O$$

Mixed with aqueous ammonia the aldehyde is converted in[to] valeral ammonia, and this digested with hydrocyanic acid an[d] hydrochloric acid is converted into leucine—

* Fownes, *Manual*, p. 319.
‡ Watts, *Dictionary*, Vol. III. p. 574.
† Fownes, p. 319.
§ *Ib.* Vol. V. p. 973.

$$C_4H_9.CHO + NH_3 = C_4H_9.CH\,(NH_2)\,OH\ *$$
valeral ammonia.

$$H_9.CH\,(NH_2).OH + HCN + H_2O = C_4H_9.CH \begin{cases} NH_2 \\ COOH \end{cases} + NH_3\ \dagger$$
leucine

his is the usual way of obtaining leucine artificially; but Tiemann as shown‡ that the amido-acids, both of the fatty and aromatic eries, may be obtained by converting the aldehydes and ketones ito cyanhydrins and then into amido-nitriles or cyanamides. We iay consequently also have the following changes:

$$C_4H_9.CHO + HCN = C_4H_9.CH \begin{cases} OH \\ CN \end{cases}$$
pentene cyanhydrin

$$C_4H_9.CH \begin{cases} OH \\ CN \end{cases} + NH_3 = C_4H_9.CH \begin{cases} NH_2 \\ CN \end{cases} + H_2O$$
pentene cyanamide

* Watts, *Dictionary*, Vol. v. p. 974. † Fownes, p. 385.

‡ *Berichte der deutsch. chem. Gesell.* XIV. s. 1985. "The amido acids of the tty series are easily obtained by the familiar reactions which take place on treating dehyde ammonia with hydrochloric and hydrocyanic acids, and which led Strecker the discovery of alanine. . . . . The reactions indicated by Strecker take place nquestionably according to the following general formulæ:

$$R \ldots C \begin{cases} NH_2 \\ H \\ OH \end{cases} + HCN = R \ldots C \begin{cases} NH_2 \\ H \\ CN \end{cases} + H_2O,$$

nd

$$R \ldots C \begin{cases} NH_2 \\ H \\ CN \end{cases} + 2H_2O + HCl = R \ldots CH\,(NH_2) \ldots COOH + H_4NCl.$$

he question arises, whether the cyanamide

$$R \ldots C \begin{cases} NH_2 \\ H \\ CN \end{cases}$$

ould not be obtained more readily from the cyanhydrides of the aldehydes

$$R \ldots C \begin{cases} CN \\ H \\ OH \end{cases}$$

y digesting them with ammonia, expecting the ultimate change to be as follows:

$$R \ldots C \begin{cases} CN \\ H \\ OH \end{cases} + NH_3 = R \ldots C \begin{cases} CN \\ H \\ NH_2 \end{cases} + H_2O.$$

he truth of this supposition has been confirmed by experiment."—*Berichte*, XIII. , 382.

$$C_4H_9.CH\begin{cases}NH_2\\CN\end{cases}+2H_2O=C_4H_9.CH\begin{cases}NH_2\\COOH\end{cases}+NH_3.$$

leucine

Leucine prepared in this way is not quite identical with anima leucine. The explanation I venture to suggest is that just as sarco lactic acid is a compound of ethidene and ethene lactic acids, s leucine is a compound of two or more amido-acids of which a secon may be prepared in the following manner :—

By the oxidation of the pentyl alcohol, diethyl carbinol, we ob tain diethyl ketone*—

$$C_5H_{11}.HO+O=H_2O+(C_2H_5)_2CO$$

diethyl carbinol — diethyl ketone

Tieman† has shown that this with hydrocyanic acid is converte into a cyanhydrin, which, acted upon by ammonia and then by a acid, produces leucine—

$$(C_2H_5)_2CO+HCN=(C_2H_5)\,C\begin{cases}OH\\CN\end{cases}$$

diethyl-ketone-cyanhydrin

$$(C_2H_5)_2C\begin{cases}OH\\CN\end{cases}+NH_3=H_2O+(C_2H_5)_2C\begin{cases}NH_2\\CN\end{cases}$$

cyanhydrin

$$(C_2H_5)_2C\begin{cases}NH_2\\CN\end{cases}+2H_2O=(C_2H_5)_2C\begin{cases}NH_2\\COOH\end{cases}+NH_3$$

$$=C_5H_{10}\begin{cases}NH_2\\COOH\end{cases}+NH_3$$

amido-diethyl acetic acid
or leucine

3. Similarly by the oxidation of propyl alcohol, propioni aldehyde is obtained, which may be converted into propene cyan hydrin‡—

$$C_2H_5.CHO\;+\;HCN=C_2H_5.CH\begin{cases}OH\\CN\end{cases}$$

propionic aldehyde — propene cyanhydrin

By oxidation of isopropyl alcohol, dimethyl ketone or aceton

* Fownes, p. 153.

† *Berichte*, XIV. s. 1975.

‡ Fownes, p. 329.

may be obtained and then converted into dimethyl-ketone-cyanhydrin*—

$$(CH_3)_2CO + HCN = (CH_3)_2 C \begin{cases} OH \\ CN \end{cases}$$

acetone dimethyl-ketone-cyanhydrin

Further on we shall see how these are related to glutamic acid.

4. Methene cyanhydrin, the first of the series, will be obtained by similar reactions—

$$H.CHO + HCN = CH_2 \begin{cases} OH \\ CN \end{cases}$$

methyl aldehyde methene cyanhydrin

$$CH_2 (OH)_2 + HCN = CH_2 \begin{cases} OH \\ CN \end{cases} + H_2O$$

methene glycol

$$H_2.CO + HCN = H_2C \begin{cases} OH \\ CN \end{cases}$$

formic acetone

5. Glycerine is a constituent of the animal organism, existing chiefly in fat combined with hydrocyanic acid, it forms cyanhydrins—

$$C_3H_5 (HO)_3 + HCN = C_3H_5 \begin{cases} OH \\ OH \\ CN \end{cases} + H_2O$$

glycerine monocyanhydrin

6. Benzoic aldehyde is obtained by oxidising albumen, fibrin, &c, by the action of manganese dioxide and sulphuric acid†, and hippuric acid, from which benzoic acid may be obtained, is a product of the animal organism.

7. Mandelic cyanhydrin can be obtained by treating benzoic aldehyde (obtained by oxidising benzyl alcohol) with hydrocyanic acid‡—

$$C_6H_5.CHO + HCN = C_6H_5.CH \begin{cases} OH \\ CN \end{cases}$$

mandelic cyanhydrin

* Fownes, p. 329. † *Ib.* p. 508.
‡ *Berichte der deutsch. chem. Gesell.* XIV. s. 1967.

which, treated with hydrochloric acid, gives mandelic acid*—

$$C_6H_5.CH\begin{cases}OH\\CN\end{cases} + 2H_2O = \underset{\text{mandelic acid}}{C_6H_5.CH\begin{cases}OH\\COOH\end{cases}} + NH_3$$

and mandelic acid oxidised is converted into benzoic acid†—

$$C_6H_5.CH\begin{cases}OH\\COOH\end{cases} + O_3 = \underset{\text{benzoic acid}}{C_6H_5.CO_2H} + CO_2 + H_2O$$

8. Sulphur exists as an animal product in the form of a sulphite in taurine, and the acid sulphites of the alkali metals form crystalline compounds with the aldehydes‡.

9. Taurine, a product of the animal organism, may be obtained in the laboratory by heating ammonium isethionate to 210°—220° C.§—

$$\underset{\text{ammonium isethionate}}{C_2H_4\begin{cases}OH\\SO_3NH_4\end{cases}} = \underset{\text{Taurine}}{C_2H_4\begin{cases}NH_2\\SO_3H\end{cases}} + H_2O$$

Isethionic acid being obtained from the oxidation of ethene mono-thiocyanate or ethene mono-thiohydrate||—

$$\underset{\text{ethene mono-thiohydrate}}{C_2H_4\begin{cases}OH\\SH\end{cases}} + O_3 = \underset{\text{isethionic acid}}{C_2H_4\begin{cases}OH\\SO_3H\end{cases}}$$

$$\underset{\text{ethene mono-thiocyanate}}{C_2H_4\begin{cases}HO\\CNS\end{cases}} + O_3 + 2H_2O = \underset{\text{isethionic acid}}{C_2H_4\begin{cases}OH\\SO_3H\end{cases}} + CO_2 + NH_3$$

10. On decomposing albumen it yields tyrosine (a para derivation of benzene), and a much larger percentage of leucine.

11. Some compounds of cyanogen by condensation of three molecules form new compounds.

$$\underset{\text{cyanic acid}}{3CNOH} = \underset{\text{cyanuric acid}}{C_3N_3O_3H_3}$$

* Fownes, p. 537. † *Ib.* p. 538. ‡ *Ib.* p. 248. § *Ib.* p. 173. || *Ib.* p. 173.

Let us now assume that albumen is a compound of cyanhydrins, and that we must have either single molecules of each or multiples of three, and that mandelic cyanhydrin is the one from which tyrosine is obtained. If we take it as the unit we must have in the first instance one molecule of mandelic cyanhydrin with three of each of those cyanhydrins from which leucine can be derived—

$$\underset{\text{mandelic cyanhydrin}}{\left(C_6H_5.CH\begin{cases}OH\\CN\end{cases}\right)} + \underset{\text{diethyl-ketone cyanhydrin}}{\left((C_2H_5)_2C\begin{cases}OH\\CN\end{cases}\right)_3} \underset{\text{pentene cyanhydrin}}{\left(C_5H_{10}\begin{cases}HO\\CN\end{cases}\right)_3}$$

To approximate to Lieberkühn's formula it is quite evident that of the remaining cyanhydrins to which reference has been made a large proportion of the lowest in the series must be taken to satisfy the numbers. Taking then nine molecules of methene cyanhydrin and one each of the remainder we have

$$\underset{\text{methene cyanhyd.}}{\left(CH_2\begin{cases}OH\\CN\end{cases}\right)_9} + \underset{\text{dimethyl ketone cyanhydrin}}{\left((CH_3)_2C\begin{cases}OH\\CN\end{cases}\right)} \underset{\text{propene cyanhydrin}}{\left(C_2H_5.CH\begin{cases}OH\\CN\end{cases}\right)} + \underset{\text{glycerin cyanhydrin}}{\left(C_3H_5\begin{cases}OH\\OH\\CN\end{cases}\right)}$$

$$+ \underset{\text{ethidene cyanhydrin}}{\left(CH_3.CH\begin{cases}OH\\CN\end{cases}\right)} \underset{\text{ethene cyanhydrin}}{\left(C_2H_4\begin{cases}OH\\CN\end{cases}\right)} + \underset{\text{sulphurous acid}}{H_2SO_3}$$

and the whole compound becomes

$$ {}_6\begin{cases}OH\\CN\end{cases}\Bigg)\left(C_5H_{10}\begin{cases}OH\\CN\end{cases}\right)_6\left(CH_2\begin{cases}OH\\CN\end{cases}\right)_9\left(C_3H_6\begin{cases}OH\\CN\end{cases}\right)_2\left(C_3H_5\begin{cases}OH\\OH\\CN\end{cases}\right)\left(C_2H_4\begin{cases}OH\\CN\end{cases}\right)_2H_2SO_3$$

$$= C_{80}H_{133}N_{21}O_{25}S$$

three molecules of this undergoing condensation will give as the composition of albumen

$$\underset{\text{albumen}}{C_{240}H_{399}N_{63}O_{75}S_3}$$

which differs from Schützenberger's formula, $C_{240}H_{387}N_{65}O_{75}S_3$, only in the small amounts of hydrogen and nitrogen.

From these cyanhydrins or their antecedent aldehydes, or from the amido-nitriles, many products having the same composition as those occurring in the animal economy may be derived.

(1) From pentene cyanhydrin, as I have shown, leucine may be formed.

(2) The formation of lactic acid from ethidene and ethene cyanhydrins has also been given.

Alanine can also be derived from these compounds:

$$\underset{\text{ethene or ethidene cyanhydrin}}{C_2H_4\begin{cases}OH\\CN\end{cases}} + NH_3 = \underset{\text{ethene or ethidene cyanamide}}{C_2H_4\begin{cases}NH_2\\CN\end{cases}} + H_2O$$

$$C_2H_4\begin{cases}NH_2\\CN\end{cases} + 2H_2O = \underset{\text{alanine}}{C_2H_4\begin{cases}NH_2\\COOH\end{cases}} + NH_3\,^*$$

(3) Similarly from methene cyanhydrin glycocine should be obtained—

$$\underset{\text{methene cyanhyd}}{CH_2\begin{cases}OH\\CN\end{cases}} + NH_3 = \underset{\text{methene cyanamide}}{CH_2\begin{cases}NH_2\\CN\end{cases}} + H_2O$$

$$CH_2\begin{cases}NH_2\\CN\end{cases} + 2H_2O = \underset{\text{glycocine}}{CH_2\begin{cases}NH_2\\COOH\end{cases}} + NH_3$$

(4) Mandelic cyanhydrin is decomposed into its generators benzaldehyde and hydrocyanic acid even by warm water†. And by combining an amido-nitrile with benzaldehyde and treating the resulting compound with an acid the mode of formation of tyrosine seems to be indicated—

$$\underset{\text{benzaldehyde}}{C_6H_5 . CHO} + \underset{\text{methene cyanamide}}{CH_2\begin{cases}NH_2\\CN\end{cases}} + 2H_2O = \underset{\text{tyrosine}}{C_9H_{11}NO_3} + NH_3$$

(5) The formation of sarcosine in dead muscle would appear to be as follows:

$$\underset{\text{methene cyanhyd.}}{CH_2\begin{cases}OH\\CN\end{cases}} + NH_2 . CH_3 = \underset{\text{nitrile}}{CH_2\begin{cases}NH . CH_3\\CN\end{cases}} + H_2O$$

* Fownes, p. 381.

† Miller's *Chemistry*. 1880. Part III. p. 757.

and the nitrile acted upon by acids gives

$$\underset{\text{nitrile}}{CH_2\begin{cases}NH \cdot CH_3\\CN\end{cases}} + 2H_2O = \underset{\text{sarcosine}}{CH_2\begin{cases}NH \cdot CH_3\\COOH\end{cases}} + NH_3$$

The formation of aspartic and glutamic acids from the amido-nitriles will be considered further on.

If such a compound of cyanhydrins as the one I have indicated for albumen were treated with dilute acids or alkalis, the changes which would take place at once suggest an explanation of the relationship which exists between myosin, acid albumen and alkali albumen*.

Having shown that the cyanhydrins can be obtained from the various alcohols, the question naturally suggests itself, What is the destination of ordinary alcohol when taken into the stomach? I will not here discuss the many changes it may undergo in the system, but as it is a well-known fact that life has been sustained, under certain conditions, for a considerable period on alcohol and water alone, the probability of its conversion into aldehyde and cyanhydrin, so forming a constituent of albumen, and then being further converted either (i) into lactic acid, and so into carbonic acid and water, or (ii) into ethidene cyanamide and then into alanine, or, as I shall show further on, into asparagine, seems to me to be a justifiable assumption, and perfectly intelligible.

On the Conversion of Leucine and Glycocine into Urea.

Having shown how leucine and glycocine may be derived from the alcohols, I will now discuss the question, What becomes of them when introduced into the alimentary canal? and turning to physiological experiments, and pathological results, I find the following. In his classical work on Physiology, Dr Foster states: "One result of the action of the pancreatic juice is the formation of considerable quantities of leucin and tyrosin. In dealing with the statistics of nutrition, our attention will be drawn to the fact that the introduction of proteid matter into the alimentary canal is followed by a large and rapid excretion of urea, suggesting the idea that a certain part of the total quantity of the urea normally secreted comes from a direct metabolism of the proteids of the food, without these really forming a part of the tissues of the body. We do not know to what extent normal pancreatic digestion has for

* See Foster's *Physiology*. 3rd Ed. pp. 62 and 651-3.

its product leucin, and its companion tyrosin; but if, especially when a meal rich in proteids has been taken, a considerable quantity of leucin is formed, we can perceive an easy and direct source of urea, provided that the metabolism of the body is capable of converting leucin into urea. That the body can effect this change is shewn by the fact that leucin, when introduced into the alimentary canal in even large quantities, does reappear in the urine as urea; that is, the urine contains no leucin, but its urea is proportionately increased; and the same is probably the case with tyrosin. Now the leucin formed in the alimentary canal is carried by the portal blood straight to the liver; and the liver, unlike other glandular organs, does, even in a perfectly normal state of things, contain urea. We are thus led to the view that among the numerous metabolic events which occur in the hepatic cells, the formation of urea out of leucin or out of other antecedents may be ranked as one. Probable, however, as this view may seem, it has not as yet been established as a fact."

"The view that leucin is transformed into urea lands us, however, in very considerable difficulties. Leucin, as we know, is amido-caproic acid; and, with our present chemical knowledge, we can conceive of no other way in which leucin can be converted into urea than by the complete reduction of the former to the ammonia condition, and a reconstruction of the latter out of the ammonia so formed. We have a somewhat parallel case in glycocoll (or glycocine). This, which is amido-acetic acid, when introduced into the alimentary canal also reappears as urea; here too, a reconstruction of urea out of an ammonia phase must take place*."

Again,

"To ascertain the influence of the liver in the formation of urea, Solnikoff has established a direct connexion between the portal and jugular veins by means of an india-rubber tube, an operation which, if carefully performed, is borne with impunity. Vascular pressure at first lowered, soon returns to the normal. The urinary secretion was, however, completely arrested, and was not re-established until urea had been injected into the veins of the animal. The only effect of these injections was to raise the vascular pressure. The results were in no way modified by the preceding section of the splanchnic nerves. If on the other hand, instead of the portal, the crural vein is placed in communication with the jugular vein, the urinary secretion is unchanged. It is thus clear that the urea which passes out of the system by the kidneys, enters the circulation with the blood which issues from the liver†."

* Foster's *Physiology*. 3rd Ed. pp. 404, 405.
† *Lancet*, Dec. 3, 1881, p. 971.

So much for the physiological side. Let us turn to the pathological. Frerichs* pointed out that in acute atrophy of the liver, the urine contains a large quantity of leucine and tyrosine, and that urea almost or entirely disappears.

It is evident from these statements, that leucine, when introduced into the alimentary canal of a healthy animal, is converted into urea, and that the conversion takes place in the liver. But, if so, what becomes of the residue? Is it possible that it is converted into an alcohol, and thence into a cyanhydrin, to undergo the series of changes I have just described; the urea being eliminated by the kidneys? Now, there is a compound having the same ultimate composition as leucine, from which both urea and amylic alcohol can be obtained, viz. isopentyl carbamate or urethane—

$$\underset{\text{leucine}}{C_5H_{10}\begin{cases}NH_2\\COOH\end{cases}} = C_6H_{13}NO_2 = \underset{\substack{\text{isopentyl carbamate}\\\text{or urethane}}}{CO\begin{cases}NH_2\\OC_5H_{11}\end{cases}}$$

This substance acted upon by ammonia is decomposed into amyl alcohol and urea†—

$$\underset{\text{isopentyl carbamate}}{CO\begin{cases}NH_2\\OC_5H_{11}\end{cases}} + NH_3 = \underset{\text{amylic alcohol}}{C_5H_{11}.HO} + \underset{\text{urea}}{CO\begin{cases}NH_2\\NH_2\end{cases}}$$

If, then, we assume that during pancreatic digestion (and I will presently indicate how the change may take place) there is a molecular transformation of leucine into a carbamate, that this on passing into the liver comes in contact with ammonia, the product of tissue disintegration, and so is converted into urea and alcohol, we have a complete series of transformations which may take place in the animal system, and the question as to the formation of urea is solved.

The same series of changes holds good for alanine—

$$\underset{\text{alanine}}{C_2H_4\begin{cases}NH_2\\COOH\end{cases}} \text{ is converted into } \underset{\text{ethyl urethane}}{CO\begin{cases}NH_2\\OC_2H_5\end{cases}}$$

The urethane combining with ammonia is converted into alcohol and urea—

$$CO\begin{cases}NH_2\\OC_2H_5\end{cases} + NH_3 = \underset{\text{alcohol}}{C_2H_5.HO} + \underset{\text{urea}}{CO\begin{cases}NH_2\\NH_2\end{cases}}$$

* *Klinik der Leberkrankheiten*, 1858. s. 206.

† Fownes, p. 390.

Oxidised, the alcohol is turned into aldehyde—

$$C_2H_5.HO + O \quad = \underset{\text{ethyl aldehyde}}{CH_3.CHO} + H_2O$$

The aldehyde combining with hydrocyanic acid is converted into the cyanhydrin, and becomes a constituent of albumen; and glycol, however formed, undergoes a similar transformation—

$$CH_3.CHO + HCN = \underset{\text{ethidene cyanhydrin}}{CH_3.CH\begin{cases}OH\\CN\end{cases}}$$

and

$$\underset{\text{glycol}}{C_2H_4(HO)_2} + HCN = \underset{\text{ethene cyanhydrin}}{C_2H_4\begin{cases}OH\\CN\end{cases}} + H_2O$$

The cyanhydrin brought in contact with ammonia becomes an amido-nitrile, or cyanamide—

$$C_2H_4\begin{cases}OH\\CN\end{cases} + NH_3 = \underset{\substack{\text{ethene or ethidene}\\\text{cyanamide}}}{C_2H_4\begin{cases}NH_2\\CN\end{cases}} + H_2O$$

and the nitrile by the action of acids or alkalies is converted into an amido-acid, or alanine—

$$C_2H_4\begin{cases}NH_2\\CN\end{cases} + 2H_2O = \underset{\text{alanine}}{C_2H_4\begin{cases}NH_2\\COOH\end{cases}} + NH_3$$

Again, we have the same series of changes for glycocine. In the duodenum, it is converted into carbamate—

$$\underset{\text{glycocine}}{CH_2\begin{cases}NH_2\\CO_2H\end{cases}} \text{ is transformed into } \underset{\substack{\text{methyl carbamate}\\\text{or urethane}}}{CO\begin{cases}NH_2\\OCH_3\end{cases}}$$

$$CO\begin{cases}NH_2\\OCH\end{cases} + NH_3 = \underset{\text{methyl alcohol}}{CH_3.HO} + \underset{\text{urea}}{CO\begin{cases}NH_2\\NH_2\end{cases}}$$

$$CH_3.HO + O = \underset{\text{methyl aldehyde}}{H.CHO} + H_2O$$

$$H.CHO + HCN = CH_2\begin{cases}OH\\CN\end{cases}$$
methene cyanhydrin

$$CH_2\begin{cases}OH\\CN\end{cases} + NH_3 = CH_2\begin{cases}NH_2\\CN\end{cases} + H_2O$$
methene cyanamide

$$CH_2\begin{cases}NH_2\\CN\end{cases} + 2H_2O = CH_2\begin{cases}NH_2\\COOH\end{cases} + NH_3$$
glycocine

These changes, I think, exemplify very well the way in which the wants of the system are supplied; nitrogen appearing in the formulæ as ammonia, NH_3, when it is the result of disintegration of tissue, and as a cyanide, or nitrile, when required for the formation of a tissue. For instance, in the series of changes given for the formation of alanine, if ethidene cyanhydrin were decomposed without coming in contact with NH_3, it would itself at once be transformed into ammonia and lactic acid—

$$CH_3.CH\begin{cases}OH\\CN\end{cases} + 2H_2O = CH_3.CH\begin{cases}OH\\COOH\end{cases} + NH_3$$
ethidene cyanhydrin lactic acid

the lactic acid being by oxidation converted into carbonic acid and water.

$$CH_3.CH\begin{cases}OH\\COOH\end{cases} + 3O_2 = 3CO_2 + 3H_2O$$
lactic acid

the ammonia combining with a urethane to form urea and an alcohol.

Or the lactic acid may pass through the intermediate stages of (i) formic acid and acetic acid, (ii) aldehyde and carbonic acid, or (iii) oxalic acid—

$$CH_3.CH\begin{cases}OH\\COOH\end{cases} + O = H.COOH + CH_3.COOH$$
lactic acid formic acid acetic acid

$$CH_3.CH\begin{cases}OH\\COOH\end{cases} + O = CH_3.CHO + CO_2 + H_2O$$
lactic acid aldehyde

$$CH_3.CH\begin{cases}OH\\COOH\end{cases} + O_3 = C_2O_2(HO)_2 + 2H_2O$$
lactic acid oxalic acid

Similarly, methene cyanhydrin, instead of forming glycocine, would be converted into glycollic acid.

$$CH_2\begin{cases}OH\\CN\end{cases} + 2H_2O = CH_2\begin{cases}OH\\COOH\end{cases} + NH_3$$

methene cyanhydrin — glycollic acid

the glycollic acid being then converted by oxidation either into formic acid, oxalic acid, or carbonic acid.

$$CH_2\begin{cases}OH\\COOH\end{cases} + O = 2H.COOH$$

glycollic acid — formic acid

$$CH_2\begin{cases}OH\\COOH\end{cases} + O_2 = C_2O_2(OH)_2 + H_2O$$

glycollic acid — oxalic acid

or

$$CH_2\begin{cases}OH\\COOH\end{cases} + O_3 = 2CO_2 + 2H_2O$$

The next question for consideration is what change takes place when the carbamate is brought in contact with some other body instead of NH_3. Taking ethidene cyanamide, for instance, the corresponding alcohol is liberated and a cyanide formed—

$$CO\begin{cases}NH_2\\OCH_3\end{cases} + C_2H_4\begin{cases}NH_2\\CN\end{cases} = CH_3.HO + C_2H_4\begin{cases}NH.CONH_2\\CN\end{cases}$$

methyl urethane — ethidene cyanamide — cyanide

or $$= CH_3.HO + C_2H_3(NH_2)\begin{cases}CO(NH_2)\\CN\end{cases}\ *$$

methyl alcohol — cyanide

which by hydration (? with acids or alkalis) gives the formula for asparagine—

$$C_2H_3(NH_2)\begin{cases}CO(NH_2)\\CN\end{cases} + 2H_2O = C_2H_3(NH_2)\begin{cases}CONH_2\\COOH\end{cases} + NH_3$$

asparagine

The other urethanes would, with ethidene cyanamide, present similar formulæ for conversion into asparagine.

* Several compounds, when converted into cyanides and thence into acids, undergo molecular transposition, e.g. allyl iodide, and ethidene dibromide. See Fownes, pp. 307, 347.

Again, if propene cyanamide is brought into contact with any of the urethanes, we shall have in a similar manner the amide of glutamic acid produced—

$$\underset{\text{urethane}}{CO\begin{cases}NH_2\\OCH_3\end{cases}} + \underset{\text{propene cyanamide}}{C_3H_6\begin{cases}NH\\CN\end{cases}} = CH_3.HO + \underset{\text{cyanide}}{C_3H_6\begin{cases}NH.CONH_2\\CN\end{cases}}$$

$$\text{or} = CH_3.HO + \underset{\text{cyanide}}{C_3H_5(NH_2)\begin{cases}CO(NH_2)\\CN\end{cases}}\ *$$

$$C_3H_5(NH_2)\begin{cases}CO(NH_2)\\CN\end{cases} + 2H_2O = \underset{\text{amide of glutamic acid}}{C_3H_5(NH_2)\begin{cases}CO(NH_2)\\COOH\end{cases}} + NH_3$$

Lastly, and most interesting of all is the combination which would appear to result if the urethane is brought in contact with HCN instead of NH_3. By condensation and combination of three molecules the formula again indicates the liberation of the corresponding alcohol with the formation of uric acid and cyanamide—

$$3CO\begin{cases}NH_2\\OCH_3\end{cases} + 3HCN = \underset{\text{methyl alcohol}}{3CH_3.HO} + \underset{\text{uric acid}}{C_5H_4N_4O_3} + \underset{\text{cyanamide}}{CN.NH_2}$$

the cyanamide combining with water to form urea—

$$\underset{\text{cyanamide}}{CN.NH_2} + H_2O = \underset{\text{urea}}{CO\begin{cases}NH_2\\NH_2\end{cases}}$$

Similarly for pentyl urethane we have

$$\underset{\text{isopentyl urethane}}{3CO\begin{cases}NH_2\\OC_5H_{11}\end{cases}} + 3HCN = \underset{\text{amyl alcohol}}{3C_5H_{11}.HO} + \underset{\text{uric acid}}{C_5H_4N_4O_3} + \underset{\text{cyanamide}}{CN.NH_2}$$

Having shown that if a urethane is produced from an amido-acid these other changes may follow, I will now endeavour to point out how the transformation is possibly effected.

* See note on opposite page.

Taking glycocine, we find that its antecedent $CH_2\left\{\begin{matrix}OH\\CN\end{matrix}\right.$ combining with the antecedents of cholalic acid, is poured out into the duodenum from the bile duct as glycocholic acid—

$$\underset{\text{methene cyanhydrin}}{CH_2\left\{\begin{matrix}OH\\CN\end{matrix}\right.} + \underset{\text{cholalic acid}}{C_{24}\,H_{40}\,O_5} = \underset{\text{glycocholic acid}}{C_{26}\,H_{43}\,NO_6}$$

This acid is decomposed in the intestines, so that "after it has served its purpose in digestion, the ammonia compound is returned into the blood*." The bile acid by hydration is decomposed as follows:

$$\underset{\text{glycocholic acid}}{C_{24}\,H_{43}\,NO_6} + H_2O = \underset{\text{glycocine}}{CH_2\,(NH_2)\,COOH} + \underset{\text{cholalic acid}}{C_{24}\,H_{40}\,O_5}$$

and is brought in contact with glucose produced by the action of the digestive fluids on starch and sugar. Now we know that various kinds of sugar in the presence of water and certain ferments, viz. albuminous substances in a peculiar state of decomposition, are converted into lactic acid†—

$$\underset{\text{glucose}}{C_6\,H_{12}\,O_6} = \underset{\text{lactic acid}}{2C_3\,H_6\,O_3}$$

$$= 2CH_3.CH\left\{\begin{matrix}OH\\COOH\end{matrix}\right.$$

that is, the glucose either undergoes a molecular transformation, or it enters into chemical combination with the nitrogenous or albuminous body. If glycocine be the nitrogenous body present, we can hardly assume in either case that it will remain unchanged, but we may expect that it will also experience under certain conditions a similar change to the glucose. This transformation, I venture to suggest, is into methyl urethane—

$$\underset{\text{glycocine}}{2CH_2\left\{\begin{matrix}NH_2\\COOH\end{matrix}\right.} + \underset{\text{glucose}}{C_6\,H_{12}\,O_6} = \underset{\text{lactic acid}}{2C_3\,H_6\,O_3} + \underset{\text{methyl urethane}}{2CO\left\{\begin{matrix}NH_2\\OCH_3\end{matrix}\right.}$$

$$= 2\,CH_3.CH\left\{\begin{matrix}OH\\COOH\end{matrix}\right. + 2CO\left\{\begin{matrix}NH_2\\OCH_3\end{matrix}\right.$$

* Foster's *Physiology.* 3rd Ed. p. 231.
† Fownes, *Chemistry*, p. 324.

Similarly

$$2C_5H_{10}\begin{cases}NH_2\\COOH\end{cases} + C_6H_{12}O_6 = 2\left\{C_2H_4\begin{cases}OH\\COOH\end{cases} + CO\begin{cases}NH_2\\OC_5H_{11}\end{cases}\right\}$$

leucine glucose lactic acid urethane

$$2C_2H_4\begin{cases}NH_2\\COOH\end{cases} + C_6H_{12}O_6 = 2\left\{ \text{———} + CO\begin{cases}NH_2\\OC_2H_5\end{cases}\right\}$$

alanine urethane

The questions at once arise, Why should not the amido-acid itself be transformed into the carbamate, and why should the glucose be introduced? Glycocine heated with baryta is converted into carbon dioxide and methylamine; heated with benzoic acid in a sealed tube, hippuric acid is formed, water being eliminated—

$$CH_2(NH_2)COOH + C_6H_5.COOH = C_9H_9NO_3 + H_2O$$

glycocine benzoic acid hippuric acid

Consequently when heated it is transformed in some way. We find glucose present in the duodenum and glycogen existing in the liver, the amount being dependent upon the amount of starch and sugar introduced into the alimentary canal, and these substances must have some function to perform. Moreover it appears that when leucine is heated with soda-lime it will not yield all its nitrogen as ammonia unless previously mixed with sugar*. Then again if we turn to the vegetable kingdom, the experiments of Nägeli† show that albumen can be formed by fungi whose nutriment consists exclusively of sugar mixed with ammonia. These considerations would apparently furnish some clue to the changes which take place in glucose previous to its transformation into lactic acid or into alcohol; but this point, however, may well be reserved until experiments in the laboratory shall have corroborated or falsified the hypothesis which I have here submitted for consideration. My friend Mr A. Scott has already made some experiments for me in the University laboratory, but too few in number to afford as yet any definite result. I have to tender my thanks to him, as well as to Mr Pattison Muir and Mr Sheridan Lea, for very valuable suggestions I have obtained in discussions I have been permitted to have with them on the subject matter of this paper.

* Watts, *Dictionary*. 2nd Suppl. p. 734.
† *Sitzungsb. d. Bayr. Acad. d. Wiss.* 1879.

(2) Mr R. Irwin Lynch exhibited to the Society a plant of Duboisia myoporoides, from the Botanic Garden, and also some dried specimens to shew the inflorescence.

This plant, quite recently introduced by seeds from the Baron von Mueller, is of interest as the source of a new alkaloid, probably of considerable medicinal value. It is called Duboisin, and at Sydney and Brisbane is now used in ophthalmic cases instead of atropine. It is said to be superior and is much more powerful and rapid, is less irritating, and is useful when the patient does not respond properly to atropine.

This Duboisia myoporoides forms a small tree about 20 ft. high, and is native of Australia, near Sydney and at Cape York; it is also native of New Guinea and New Caledonia. It has small, pale lilac or white flowers, and belongs to the Solaneæ.

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(3) *On a method of deriving formulæ in Elliptic Functions.* By J. W. L. Glaisher, M.A.

[Read *November* 28, 1881.]

If $\operatorname{sn} u$, when the modulus is put equal to unity, be denoted by $\operatorname{sn}_1 u$, we have

$$\operatorname{sn}_1 (u-v) = \frac{\operatorname{sn}_1 u - \operatorname{sn}_1 v}{1-\operatorname{sn}_1 u \operatorname{sn}_1 v},$$

and also, the modulus being k,

$$k \operatorname{sn}(\alpha-\beta) \operatorname{sn}(\alpha+\beta) = \frac{k \operatorname{sn}^2\alpha - k\operatorname{sn}^2\beta}{1-k^2\operatorname{sn}^2\alpha \operatorname{sn}^2\beta};$$

if therefore we put

$$\operatorname{sn}_1 u = k\operatorname{sn}^2\alpha, \quad \operatorname{sn}_1 v = k\operatorname{sn}^2\beta,$$

we convert

$$\operatorname{sn}_1(u-v) \text{ into } k\operatorname{sn}(\alpha-\beta)\operatorname{sn}(\alpha+\beta).$$

It follows therefore that if we have any formula involving only the sn's of *differences* of quantities, if we first put $k=1$, we may replace the sn of each difference by k times the product of the sn's of the sums and differences of the quantities, *i.e.*

$$\operatorname{sn}_1(u-v) \text{ by } k\operatorname{sn}(u-v)\operatorname{sn}(u+v),$$
$$\operatorname{sn}_1(u-w) \text{ by } k\operatorname{sn}(u-w)\operatorname{sn}(u+w), \text{ \&c.}$$

thus obtaining a new formulæ which will be true for modulus k.

Thus for example, in the formula

$$\operatorname{sn}(w-x)\operatorname{sn}(y-z)+\operatorname{sn}(w-y)\operatorname{sn}(z-x)+\operatorname{sn}(w-z)\operatorname{sn}(x-y)$$
$$+k^2\operatorname{sn}(w-x)\operatorname{sn}(w-y)\operatorname{sn}(w-z)\operatorname{sn}(y-z)\operatorname{sn}(z-x)\operatorname{sn}(x-y)=0,$$

we put $k=1$, giving

$$\operatorname{sn}_1(w-x)\operatorname{sn}_1(y-z)+\operatorname{sn}_1(w-y)\operatorname{sn}_1(z-x)+\operatorname{sn}_1(w-z)\operatorname{sn}_1(x-y)$$
$$+\operatorname{sn}_1(w-x)\operatorname{sn}_1(w-y)\operatorname{sn}_1(w-z)\operatorname{sn}_1(y-z)\operatorname{sn}_1(z-x)\operatorname{sn}_1(x-y)=0,$$

whence we have

$$\begin{aligned}
&\operatorname{sn}(w-x)\operatorname{sn}(w+x)\operatorname{sn}(y-z)\operatorname{sn}(y+z)\\
&+\operatorname{sn}(w-y)\operatorname{sn}(w+y)\operatorname{sn}(z-x)\operatorname{sn}(z+x)\\
&+\operatorname{sn}(w-z)\operatorname{sn}(w+z)\operatorname{sn}(x-y)\operatorname{sn}(x+y)\\
&+k^4\operatorname{sn}(w-x)\operatorname{sn}(w+x)\operatorname{sn}(w-y)\operatorname{sn}(w+y)\operatorname{sn}(w-z)\operatorname{sn}(w+z)\\
&\times\operatorname{sn}(y-z)\operatorname{sn}(y+z)\operatorname{sn}(z-x)\operatorname{sn}(z+x)\operatorname{sn}(x-y)\operatorname{sn}(x+y)=0.
\end{aligned}$$

We may replace the twelve arguments $y-z$, $y+z$, &c. by their equivalents in the following system of equations:

$$\begin{aligned}
&y-z=\beta-\gamma, && z-x=\gamma-\alpha, && x-y=\alpha-\beta,\\
&y+z=\delta-\alpha, && z+x=\delta-\beta, && x+y=\delta-\gamma,\\
&w-x=\beta+\gamma, && w-y=\gamma+\alpha, && w-z=\alpha+\beta,\\
&w+x=\delta+\alpha, && w+y=\delta+\beta, && w+z=\delta+\gamma;
\end{aligned}$$

and it therefore follows that, in any formula involving the three arguments $y-z$, $z-x$, $x-y$, or the six arguments $y-z$, $z-x$, $x-y$, $w-x$, $w-y$, $w-z$, we may replace

$$\begin{array}{lll}
\operatorname{sn}_1(y-z) & \text{by} & k\operatorname{sn}(\beta-\gamma)\operatorname{sn}(\delta-\alpha),\\
\operatorname{sn}_1(z-x) & ,, & k\operatorname{sn}(\gamma-\alpha)\operatorname{sn}(\delta-\beta),\\
\operatorname{sn}_1(x-y) & ,, & k\operatorname{sn}(\alpha-\beta)\operatorname{sn}(\delta-\gamma),\\
\operatorname{sn}_1(w-x) & ,, & k\operatorname{sn}(\beta+\gamma)\operatorname{sn}(\delta+\alpha),\\
\operatorname{sn}_1(w-y) & ,, & k\operatorname{sn}(\gamma+\alpha)\operatorname{sn}(\delta+\beta),\\
\operatorname{sn}_1(w-z) & ,, & k\operatorname{sn}(\alpha+\beta)\operatorname{sn}(\delta+\gamma).
\end{array}$$

Thus for example from the formula

$$\operatorname{sn}(w-y)\operatorname{sn}(w-z)\operatorname{sn}(y-z)+\operatorname{sn}(w-z)\operatorname{sn}(w-x)\operatorname{sn}(z-x)$$
$$+\operatorname{sn}(w-x)\operatorname{sn}(w-y)\operatorname{sn}(x-y)+\operatorname{sn}(y-z)\operatorname{sn}(z-x)\operatorname{sn}(x-y)=0,$$

we derive

$$\begin{aligned}
&\operatorname{sn}(\beta-\gamma)\operatorname{sn}(\gamma+\alpha)\operatorname{sn}(\alpha+\beta)\operatorname{sn}(\delta-\alpha)\operatorname{sn}(\delta+\beta)\operatorname{sn}(\delta+\gamma)\\
&+\operatorname{sn}(\gamma-\alpha)\operatorname{sn}(\alpha+\beta)\operatorname{sn}(\beta+\gamma)\operatorname{sn}(\delta+\alpha)\operatorname{sn}(\delta-\beta)\operatorname{sn}(\delta+\gamma)\\
&+\operatorname{sn}(\alpha-\beta)\operatorname{sn}(\beta+\gamma)\operatorname{sn}(\gamma+\alpha)\operatorname{sn}(\delta+\alpha)\operatorname{sn}(\delta+\beta)\operatorname{sn}(\delta-\gamma)\\
&+\operatorname{sn}(\beta-\gamma)\operatorname{sn}(\gamma-\alpha)\operatorname{sn}(\alpha-\beta)\operatorname{sn}(\delta-\alpha)\operatorname{sn}(\delta-\beta)\operatorname{sn}(\delta-\gamma)=0.
\end{aligned}$$

Since $\text{sn}_1 iu = i \tan u$, a formula involving sn_1's is equivalent to a trigonometrical formula involving tangents, so that the method affords a means of deriving elliptic function formulæ involving sn's (and therefore also trigonometrical formulæ involving sines) from trigonometrical formulæ involving tangents.

In Vol. III. pp. 383—387 a method was given of deriving from a formula $\Sigma . \Pi \sin(\alpha - \beta) = 0$ a trigonometrical formula of the form $\Sigma . \Pi \sin(\alpha - \beta) \sin(\alpha + \beta) = 0$; but this method of deriving one sine-formula from another can be applied only when the number of sines multiplied together is the same in each term of the formula. But in the above method, where the original formula involves sn's, this condition is not necessary, viz. we put $k = 1$ and obtain a formula involving $\text{sn}_1(\alpha - \beta)$, &c.: we then replace $\text{sn}_1(\alpha - \beta)$, &c. by $k \,\text{sn}(\alpha - \beta)\,\text{sn}(\alpha + \beta)$, &c. and we deduce a trigonometrical formula involving sines by putting $k = 0$, and a trigonometrical formula involving tangents by putting $k = 1$, replacing α, β, &c. by $i\alpha$, $i\beta$, &c. and substituting $i \tan(\alpha - \beta)$, &c. for $\text{sn}_1(i\alpha - i\beta)$, &c.

February 20, 1882.

PROFESSOR STOKES IN THE CHAIR.

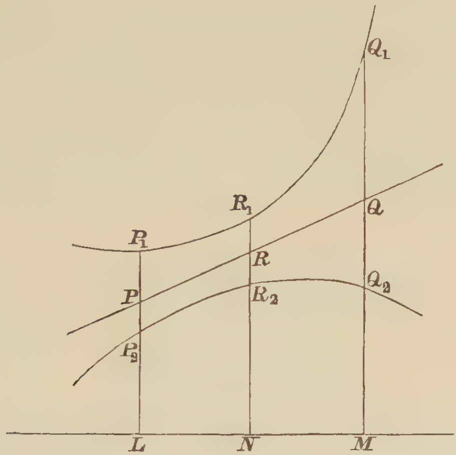
The following communications were made to the Society:

(1) *On the effect of fluctuations in a variable, upon the mean values of functions of that variable: with an application to the theory of Glacial Epochs.* By E. HILL, M.A., Tutor of St John's College.

Let a variable x be changed continuously and uniformly from a magnitude $a - b$ to another $a + b$: and let $f(x)$ be a function continuous between $f(a - b)$ and $f(a + b)$. Then if the mean of its intermediate values be taken, this will not in general coincide with $f(a)$, the value of the function for a, the mean magnitude of the variable. So, in any continuous change of a variable and of a function of that variable, the mean value of the function will not in general be the value of it for the mean magnitude of the variable.

Take a series of values of y, uniformly increasing in magnitude, such as the ordinates of the straight line PRQ. Let the corresponding values of any functions $f_1(y)$, $f_2(y)$ be represented by

the corresponding ordinates of the curves $P_1R_1Q_1$, $P_2R_2Q_2$. Suppose N middle point of LM. Then RN is the mean of the magnitudes



PL, QM, but R_1N, R_2N, are respectively less and greater than the means of P_1L, Q_1M, and of P_2L, Q_2M.

It is evident from the figure that in a curve for which $\frac{d^2f}{dx^2}$ is of constant sign, the mean of any pair of ordinates is greater or less than the ordinate from which they are equi-distant on each side, according as $\frac{d^2f}{dx^2}$ is positive or negative. Therefore for any portion of such curve, the mean value of the ordinate is greater or less than the ordinate corresponding to the mean abscissa as $\frac{d^2f}{dx^2}$ is positive or negative. I do not propose to consider cases in which $\frac{d^2f}{dx^2}$ changes sign between the limits.

Recurring to the figure, since y represents the ordinate of the line PRQ, $\frac{dy}{dx}$ is constant.

Therefore $$\frac{d^2f}{dx^2}=\frac{d^2f}{dy^2}\cdot\left(\frac{dy}{dx}\right)^2,$$

whence it follows that we may use $\frac{d^2f}{dy^2}$ as our test; and say that the

mean value of $f(y)$ for a continuous change in y exceeds or falls short of the value for the mean magnitude of y according as $\frac{d^2f}{dy^2}$ is positive or negative.

The same will be equally true if y change back again from PL to QM. Therefore it is true for oscillations of y. Thus,—If a variable perform oscillations about a mean magnitude, the mean value of a function of that variable is greater or less than the value for that mean magnitude, according as the second differential co-efficient of the function is positive or negative.

Conversely, if $u = f(y)$,

$$\frac{d^2y}{du^2} = -\frac{d^2u}{dy^2}\left(\frac{du}{dy}\right)^{-3}$$

$$= -\frac{d^2f}{dy^2}\cdot\left(\frac{df}{dy}\right)^{-3}.$$

So that if $\frac{df}{dy}$ be positive, $\frac{d^2y}{du^2}$ is opposite in sign to $\frac{d^2u}{dy^2}$ or $\frac{d^2f}{dy^2}$, and therefore if we cause f instead of y to oscillate about a mean, the resulting mean of y is less than its value for the mean magnitude of f, when f is such a function that for oscillations of y it is greater than its value for the mean magnitude of y. And *vice versa*. (This does not hold if f have a maximum or minimum value between the limits of variation.)

An important instance of this is the connection between Radiation and Temperature. According to Dulong and Petit the radiation of a body at temperature $t + \theta$ surrounded by an envelope of temperature θ is proportional to the function $a^\theta(a^t - 1)$. Here if we denote the rate of radiation by r, $\frac{dr}{dt}$ and $\frac{d^2r}{dt^2}$ are both positive. Therefore if the temperature of a body be made to oscillate, its mean radiation is greater than its radiation at the mean temperature. Conversely if its rate of radiation after being constant under a constant temperature be made to fluctuate about that rate as a mean, the mean temperature must fall below its previous constant value.

Again, if we have a body receiving heat from some external source, its temperature rises till its radiation equals its absorption. If its capacity for heat be very small, this rise will take place so rapidly as to be nearly simultaneous with the increase in heat-supply, and the radiation at any moment will nearly equal the

supply. Thus if we call h the heat-supply we have $h = k\,a^{\theta}\,(a^{t} - 1)$; $\frac{dh}{dt}$, $\frac{d^2h}{dt^2}$ are positive; and fluctuations in t about a mean, indicate an increased mean value of h; while conversely, fluctuations in h about a mean, produce a diminished mean of the values of t. Now the materials of the earth's surface have no great capacity for heat. We find that the diurnal and annual variations of heat-supply produce variations of temperature which are of equal period though somewhat retarded in phase. I conclude that *the vicissitudes of day and night, and of summer and winter, lower the mean temperature of dry land* below that which it would have if it received the same amount of heat as now, but uniformly and continuously.

The capacity for heat of water is considerable; but it parts with heat communicated to it largely by means of evaporation as well as radiation. Other things being equal the amount of this evaporation at any temperature must bear some relation to the quantity of vapour which will saturate the air at that temperature. It is usually considered to be proportional to that. The quantity is a function of the temperature which increases with it under a rapid acceleration. Let h measure the rate of heat supply, $f(t)$ and $\phi(t)$ the rate of heat-emission by radiation and evaporation respectively. Then for a body of such small capacity, or otherwise so constituted, that its emission at every moment is equal to its absorption, we have

$$h = f(t) + \phi(t),$$

where f and ϕ are functions whose first and second differential coefficients are both positive. Differentiating we get

$$\frac{d^2h}{dt^2} = f'' + \phi'',$$

and therefore positive;

$$\frac{d^2t}{dh^2} = -(f'' + \phi'')\,(f' + \phi')^{-3},$$

and therefore negative. Hence as before oscillations of temperature about a mean, require a heat-supply whose mean value is greater than that required to maintain the body at that mean temperature; and conversely, fluctuations of heat-supply about a mean produce a mean temperature which is lower than that which the mean heat-supply would maintain if constant.

It is not probable that the rays of the sun penetrate very deeply into the sea. And the lightness of watery vapour tends to make

the air which has absorbed it rise into upper regions where much of its heat will be radiated into space, while a circulation of air will be kept up, and evaporation go on continuously. I conclude that in the case of the sea as well as in that of dry land, the vicissitudes of day and night and winter and summer, lower the mean temperature.

It is requisite to know, for one problem which presents itself, which of these two, radiation or evaporation, has the largest share in producing this result. Evaporation, an explicit function $\phi(t)$ of temperature, is by the equation

$$\phi(t) + f(t) = h$$

an implicit function of the rate of heat-supply. Let us examine the sign of its second differential coefficient with respect to h. Differentiating, taking alternately h and t, as dependent variable,

$$\frac{d\phi}{dh} + f'(t)\frac{dt}{dh} = 1,$$

and

$$\phi' + f' = \frac{dh}{dt};$$

whence

$$\frac{d\phi}{dh} = 1 - f'\frac{dt}{dh}$$

$$= \frac{\phi'}{f' + \phi'},$$

and is therefore positive.

Differentiating again, with respect to h,

$$\frac{d^2\phi}{dh^2} = \frac{d}{dt}\left(\frac{\phi'}{f' + \phi'}\right)\frac{dt}{dh}$$

$$= \frac{\phi''f' - \phi'f''}{(f' + \phi')^3}$$

$$= \left(\frac{\phi''}{\phi'} - \frac{f''}{f'}\right)\frac{f'\phi'}{(f' + \phi')^3}.$$

If as above we suppose the radiation for temperature t to be proportional to $a^t - 1$, we obtain for the second term of the bracket $\log a$, and since a is, according to Dulong and Petit, 1·0077, the value of this is ·00767 approximately. Other formulæ give a lower value and Newton's makes f'' to be zero.

Various formulæ have been given for the capacity for vapour of air at various temperatures (to which we have supposed evaporation proportional). For our present purpose a rough approximation suffices. The formula

$$K(1 + 0{\cdot}014\,t)^5,$$

where K is capacity at zero centigrade (an adaptation of Young's formula) will be found to give at ordinary temperatures results within about 1 per cent. of the truth. From this we obtain for the first term of the bracket

$$\frac{\cdot 056}{1 + 0{\cdot}014\,t}.$$

Thus for all ordinary temperatures $\dfrac{d^2\phi}{dh^2}$ is positive. Similarly $\dfrac{d^2 f}{dh^2}$ is negative.

Hence then fluctuations in heat-supply about a mean increase mean evaporation and decrease mean radiation.

It is obvious that an increase in the amplitudes of such oscillations will still further increase or decrease mean values. Now an increase in the eccentricity of the earth's orbit will make the earth nearer the sun at Perihelion, and further off at Aphelion, than it is at present. Thus the difference between the temperatures of summer and winter will be intensified for a hemisphere whose winter is in Perihelion, but mitigated for the other. It appears from these investigations that these changes tend in the one case to lower the mean annual temperature, in the other case to raise it: to increase the mean annual evaporation of the one, and diminish that of the other. From increased evaporation follows increased precipitation both of rain and snow. The probable effects of this in accumulating snow, and its bearing on the problem of glacial epochs I have pointed out in a paper read before the British Association at York, and published in the *Geological Magazine* for November, 1881. As I mentioned there, some rough calculations seem to shew that this cause is capable of producing a considerable effect. I hope before long to renew these calculations by a more satisfactory method, and perhaps make them the subject of a further communication to the Society.

(2) *On the application of Quaternions, and Grassmann's Ausdehnungslehre to different kinds of uniform space.* By HOMERSHAM COX, B.A., Fellow of Trinity College.

The object of this paper is following Grassmann to establish a pure algebraical calculus, the laws of which will coincide with those of actual geometry. Such a geometry will differ from ordinary algebra in having more than one independent unit. Suppose A and B to be independent units such that there can be no relation of the form $A = xB$ when x is any ordinary algebraic quantity real or imaginary.

Then A and B are called points, and every expression of the form $pA + qB$ where p, q are numbers, positive, negative or imaginary is called a point or some multiple of a point. $pA + qB$, $2(pA + qB)$, $3(pA + qB)$ are not considered different points, but different multiples of the same point.

Thus the point $pA + qB$ varies with the ratio $p : q$ and all the points included in that expression form a singly infinite series which is called a straight line. If A, B, C be distinct quantities not connected by any linear relation, all the points included in the expression $pA + qB + rC$ form a doubly infinite series which is called a line. This may be extended to three or more dimensions. If $P = xA + yB + zC$, (x, y, z) are said to be the homogeneous coordinates of the point P, and it is shewn that $lx + my + nz = O$ is the condition P should lie on a straight line. From these definitions can be shewn to follow all the descriptive or projective properties of curves considered as the loci of points satisfying equations.

It is found that there are three ways in which the idea of distance may be introduced. If P, Q, R be single (not multiple) points such that $pP + qQ = rR$ and $\alpha = \text{dist.}\, PR$, $\beta = \text{dist.}\, RQ$, $\gamma = \text{dist.}\, PQ$ so that $\alpha + \beta = \gamma$, then one of the following sets of relations must hold

$$\left.\begin{aligned} r^2 = p^2 + q^2 + 2pq \cosh \frac{\gamma}{k} \\ p \sinh \frac{\alpha}{k} = q \sinh \frac{\beta}{k} \end{aligned}\right\} \text{..................I,}$$

$$\left.\begin{aligned} q = p + q \\ p\alpha = q\beta \end{aligned}\right\} \text{..................II,}$$

$$\left.\begin{aligned} r^2 &= p^2 + q^2 + 2pq\cos\frac{\gamma}{k} \\ p\sin\frac{\alpha}{k} &= q\sin\frac{\beta}{k} \end{aligned}\right\} \text{......................III.}$$

To I, II, III will correspond three different kinds of geometry which are, respectively, the non-Euclidean geometry of Lebatschensky and Bolyai, the ordinary geometry and spherical geometry.

The different kinds of multiplications of points are investigated assuming the distributive principle, and it is shewn that the most general conditions are

$$P^2 = Q^2 = \beta, \text{ a constant}$$

and
$$P.Q + Q.P = 2\beta\cosh\theta \quad \text{in I,}$$
$$P.Q + Q.P = 2\beta \qquad \text{in II,}$$
$$P.Q + Q.P = 2\beta\cos\theta \quad \text{in III.}$$

Different kinds of multiplication can be obtained by making further special assumptions. These are,

1st. Grassmann's outer multiplication which is (for two points) identical with the vector multiplication of Quaternions.

2nd. Grassmann's inner multiplication which is (for two points) identical with the scalar multiplication of Quaternions.

3rd. The associative Quaternion multiplication with the following laws:

$$QP^{-1} = \cosh\theta + \iota\sinh\theta \qquad \iota^2 = 1 \quad \text{in I,}$$
$$QP^{-1} = 1 + \iota\theta \qquad \iota^2 = 0 \quad \text{in II,}$$
$$QP^{-1} = \cos\theta + \iota\sin\theta \qquad \iota^2 = -1 \text{ in III,}$$

ι is in each case a specific constant peculiar to the line joining P and Q.

The addition and multiplication of these quantities ι is next considered. It is shewn that if $\iota_1\ \iota_2$ be the specific quantities peculiar to lines making an angle θ and if $r\iota = p\iota_1 + q\iota_2$

then $$r^2 = p^2 + q^2 + 2pq\cos\theta.$$

Also $$\iota_2\iota_1^{-1} = \cos\theta + O\sin\theta$$

where $$O^2 = -1,$$

and O may be identified with the point at which the lines meet.

From these formulæ assuming the associative principle, the relations between the sides and angles of a triangle can be obtained. They are in I,

$$\cosh a = \cosh b \cosh c - \sinh b \sinh c \cos A$$

$$\frac{\sinh a}{\sin A} = \frac{\sinh b}{\sin B} = \frac{\sinh c}{\sin C}$$

with others similar. In this way all the properties of the different kinds of space can be obtained by a purely algebraical method. The chief distinctions between the three cases are pointed out.

Passing to the geometry of three dimensions it is found that seven distinct imaginary quantities are required which in case I. will be identical with the symbols that occur in Sir W. Hamilton's bi-quaternions. If I, J, K be the ordinary quaternion symbols

$$XI + YJ + ZK + \sqrt{-1}\,(LI + MJ + NK)$$

will represent a line or force in space provided that

$$XL + YM + ZN = 0.$$

If this equation be not satisfied it will represent a system of forces.

Some applications are made to the theory of screws and the equation to the cylindroid is found to be

$$(p_\alpha - p_\beta)(\omega^2 - z^2)\,xy = (1 + p_\alpha p_\beta)(x^2 + y^2)\,\omega z.$$

It is therefore a surface of the fourth degree as has already been shewn by Lindemann. For points near the origin making ω nearly and x, y, z, $p\alpha$, $p\beta$ small, it becomes

$$(p_\alpha - p_\beta)\,xy = z\,(x^2 + y^2)$$

and coincides with the equation to the cylindroid in ordinary space in the form given to it by Prof. Ball. It is shewn that the properties of forces in space can be derived from those of forces meeting at a point by taking for components of force instead of x, y, z the imaginary quantities $X + L\omega$, $S + M\omega$, $Z + N\omega$

where
$$\omega^2 = -1 \text{ in I},$$
$$\omega^2 = 0 \quad \text{in II},$$
$$\omega^2 = 1 \quad \text{in III}.$$

Lastly, the systems of imaginary quantities for spaces of higher dimensions are shewn to be obtained from the commutative products of lower systems.

March 6, 1882.

DR CAMPION, VICE-PRESIDENT, IN THE CHAIR.

Mr C. T. Heycock, B.A., King's College, was balloted for and duly elected a Fellow of the Society.

The following communications were made to the Society:

(1) *The use of telescopes on dark nights.* By LORD RAYLEIGH.

In *Silliman's Journal* for 1881 Mr E. S. Holden, after quoting observations to a like effect by Sir W. Herschel, gives details of some observations recently made with a large telescope at the Washburn Observatory, from which it appears that distant objects on a dark but clear night can be seen with the telescope long after they have ceased to be visible with the naked eye. He concludes, "It appears to me that this confirmation of Herschel's experiments is important, and worth the attention of physicists. So far as I know there is no satisfactory explanation of the action of the ordinary Night-glass, nor of the similar effect when large apertures are used."

It is a well-known principle that no optical combination can increase what is called the 'apparent brightness' of a distant object, and indeed that in consequence of the inevitable loss of light by absorption and reflection the 'apparent brightness' is necessarily *diminished* by every form of telescope. Having full confidence in this principle, I was precluded from seeking the explanation of the advantage in any peculiar action of the telescope, but was driven to the conclusion that the question was one of apparent magnitude only,—that a large area of given small 'apparent brightness' must be visible against a dark ground when a small area would not be visible. The experiment was tried in the simplest possible manner by cutting crosses of various sizes out of a piece of white paper and arranging them in a dark room against a black back-ground. A feeble light proceeded from a nearly turned out gas-flame. The result proved that the visibility was a question of apparent magnitude to a greater extent than I had believed possible. A distance was readily found at which the larger crosses were plainly visible, while the smaller were quite indistinguishable. To bring the latter into view it was necessary either to increase the light considerably, to approach nearer, or lastly to use a telescope. With sufficient illumination the smallest crosses used were seen perfectly defined at the full distance.

There seems to be no doubt that the explanation is to be sought within the domain of physiological optics. It has occurred to me as possible that with the large aperture of the pupil called into play in a dark place, the focussing may be very defective on account of aberration. The illumination on the retina might then be really less in the image of a small than in the image of a large object of equal 'apparent brightness.'

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(2) *On a new form of gas battery.* By LORD RAYLEIGH.

In Grove's well-known gas battery it would seem that the only efficient part of the platinum surface is where it meets both the gas and the liquid, or at any rate meets the liquid and is very near the gas. In order to render a larger area effective I have substituted for the usual platinum plates platinum gauze resting upon the surface of the liquid in a large trough in such a manner that the upper surface is damp but not immersed. One piece is exposed to the oxygen of the air; the other forms the bottom of an enclosed space into which hydrogen is caused to flow. The area of each piece is about 20 square inches.

To test the efficiency, the current was passed through an external resistance of about 6 Ohms, including a galvanometer. Under these circumstances the permanent current was about one-fourth of that obtained when a large Daniell cell was substituted for the gas element. An inferior, but still considerable, current was observed when coal gas was used instead of hydrogen prepared from zinc.

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(3) *Further observations on the transformation of alcohol and on the formation of alcohol and urea in the living body.* By P. W. LATHAM, M.D., Downing Professor of Medicine, Physician to Addenbrooke's Hospital.

IN a previous paper (Feb. 6, 1882) I endeavoured to show that albumen is a compound of cyanhydrins, that these may be obtained from the various alcohols and ketones, and be further transformed into glycocine, leucine and other bodies which are derived from the animal organism, and last of all I suggested as a reasonable hypothesis that glycocine, leucine and other amido-bodies when introduced into the living organism undergo a molecular transformation into the metameric urethanes or alcoholic carbamates, which are then decomposed in the recognised manner into urea and the corresponding alcohol.

Such a theory, startling as at first sight it may appear, seems to offer an explanation of many obscure points, and if true to present such an important starting-point for investigating the action of various remedies in health and disease, that I venture now to bring forward one or two points which such a theory may help to explain and then to direct attention to some experiments which seem to prove the truth of the theory, in as marked a manner as though they had been devised for the purpose.

An objection which may at once be urged against the theory is, that these various alcohols, amylic, ethylic and methylic, have not been detected in the living tissues. But after the administration of ordinary ethylic alcohol internally no one has as yet been able to detect in the blood the ordinary products of its oxidation. What becomes of it then?

After the internal administration of alcohol "the weight of evidence appears to be at present in favour of its diminishing the elimination of carbonic acid: although the matter cannot be considered as entirely settled*."

"Although the matter cannot be considered absolutely settled, yet the present conclusion fairly is that alcohol lessens the excretion of urea*."

"The investigations certainly demonstrate that but a small proportion of ingested alcohol is either eliminated from or accumulated in the body, and consequently that it must be oxidised in the body and in some degree partake of the nature of a food. It has been objected to this that no one has as yet been able to detect in the blood any of the ordinary products of its oxidation; the probable reason of this is however, that the oxidation is carried, as it were, at one bound to its ultimate end, the production of water and carbonic acid†."

Duchek‡ thought that he had demonstrated the presence of aldehyde in the blood of animals poisoned with alcohol, but his experiments were really not carried far enough to prove it.

Dr Hammond's§ experiments indicate that it is a food; for he gained weight, when taking it, upon a diet, which he had previously proved insufficient by itself to maintain his bodily weight.

In such a way, so far as its transformation is concerned, may be briefly summarised all that is known respecting the history of alcohol in the living body. As alcohol it passes into the system, sooner or later it passes out, *possibly* as carbonic acid and water. As regards our present knowledge of the sojourn of oxygen in the living tissues it has been very aptly said, "We cannot as yet

* Wood's *Therapeutics*, 2nd Ed. 1877, p. 117. † *Ib.* pp. 118, 119.
‡ *Viertelj. für die pract. Heilk.*, Bd. III. 1853.
§ *Physiolog. memoirs*, Philad. 1863.

trace out the steps taken by the oxygen from the moment it slips into its intra-molecular position [in muscle] to the moment when it issues united with carbon as carbonic acid. The whole mystery of life lies hidden in the story of that progress, and for the present we must be content with simply knowing the beginning and the end*." The same may be said with equal force as regards alcohol.

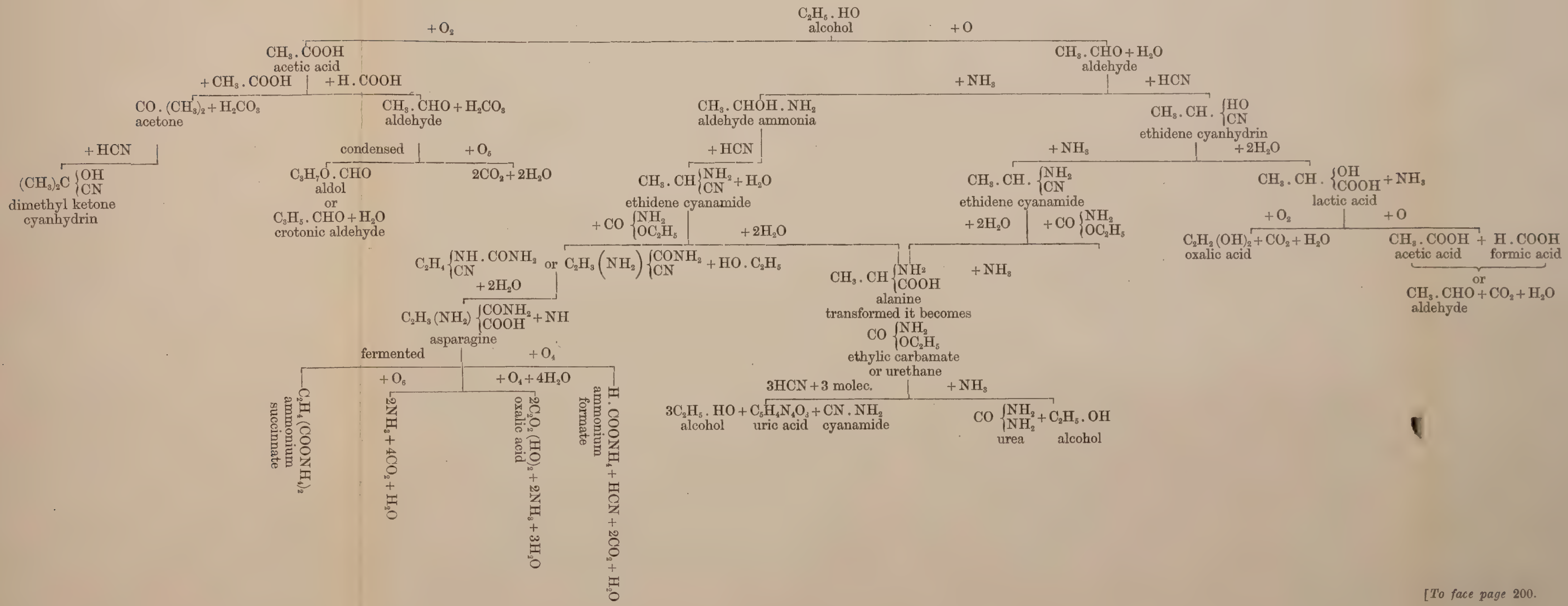
If however albumen is a compound, such as I have represented it to be, of cyanhydrins, and if the chief primary changes which take place in the tissues, result from the effects of oxidation, and of combinations with nitrogen in the form of NH_3 or HCN, then some light seems to be thrown upon the processes, and we may readily conceive how variable may be the transformations of alcohol according to the conditions under which the products are developed. The accompanying Table has been drawn up to show some few of these changes.

Taking the right-hand side of the Table we find that by oxidation, alcohol is converted into aldehyde, thence into a cyanhydrin or constituent of albumen, which hydrated becomes lactic acid. By oxidation and according to the power of the oxidising agent, this may be converted either into acetic acid and formic acid which under certain conditions combine to form aldehyde, carbonic acid and water; or the lactic acid may be converted into oxalic acid, carbonic acid and water. Or the cyanhydrin combining with ammonia may form the cyanamide or amido-nitrile, which by hydration becomes alanine. This undergoing molecular transformation is converted into an alcoholic carbamate, which leads to the formation either of urea or of uric acid, and alcohol. Again, going further back, aldehyde may combine with NH_3 forming aldehyde ammonia, thence to be converted into a cyanamide which hydrated as before becomes alanine, or combining with a urethane and then hydrated, gives the formula for asparagine, which according to the substance by which it is oxidised gives rise to the various products stated in the Table. Lastly, alcohol still further oxidised produces acetic acid; acetates and formates when heated together, produce aldehyde and a carbonate, and aldehyde by oxidation is converted into carbonic acid and water, or forming condensation products may be converted into aldol or crotonic aldehyde. The acetates again when heated alone, give rise to carbonates and acetone or dimethyl ketone, and this combining with HCN forms a cyanhydrin or constituent of albumen.

A table of similar changes can be drawn up for the other alcohols, and the products acting upon each other would give rise to an indefinite number of transformations.

* Foster's *Physiology*, 3rd Ed. 1879, p. 320.

TABLE SHOWING SOME OF THE PRODUCTS WHICH MAY BE DERIVED FROM ALCOHOL.



[To face page 200.

Many of the changes represented in the Table take place in the laboratory only under the influence of a temperature very far beyond that of the human body: urethane and ammonia for instance must be heated in sealed tubes to 180° C. to produce urea; acetates and formates must be heated to form aldehyde, the acetates must be distilled to form acetone. But may this not be taken as the measure of what is called vital force or nerve force? Compounds are formed in the living body which hitherto can be produced in the laboratory only with the aid of a high temperature. Benzoic acid for instance introduced into the alimentary canal is under certain conditions excreted as hippuric acid, which can be decomposed into benzoic acid and glycocine; but to form hippuric acid in the laboratory these two substances must be heated in a sealed tube at a high temperature. To form taurine again in the laboratory requires the heating of ammonium isethionate in a sealed tube to about 200° C. It seems therefore fair to assume that substances which out of the body require a high temperature in sealed tubes to effect their combination, may under the influence of so called vital, living, or nerve force combine in the living body at the normal temperature. Under such a supposition, we see from the Table how formation and disintegration of products are continually going on, aldehyde for instance appearing at various points to undergo further changes, and alcohol itself reappearing at the end of the Table to start a fresh series of transformations.

The decompositions in the Table show how the formation of NH_3 may take place. It remains for me to show how the HCN is produced. It appears together with ammonium formate as resulting from the oxidation of such substances as asparagine. Formic acid is also obtained by the oxidation of lactic acid, this combining with ammonia would form ammonium formate, which when heated in a sealed tube is converted into HCN and $2H_2O$.

Again, may not this hypothesis help to explain the chemical changes which take place on the contraction of a living muscle? "A considerable quantity of carbonic acid is set free......it is not accompanied by any corresponding increase in the consumption of oxygen*." If acetic acid and formic acid have been produced in the tissues from the hydration and oxidation of the cyanhydrin, then the vital action which causes the muscular contraction will cause these acids to combine and form aldehyde, carbonic acid and water, or the formic acid combining with benzoic acid from mandelic cyanhydrin, may give rise to benzoic aldehyde carbonic acid and water—

$$\underset{\text{benzoic acid}}{C_6H_5 . COOH} + \underset{\text{formic acid}}{H . COOH} = \underset{\text{benzoic aldehyde.}}{C_6H_5 . CHO} + CO_2 + H_2O.$$

* Foster's *Physiology*, 3rd Ed. p. 65.

The theory which I have here advanced depends entirely on the possibility of the amido-acids being converted in the living body into urethanes or alcoholic carbamates. Is there then any proof of such a change being effected, any proof in fact of the existence in the body of carbamic acid?

Assuming the existence of these urethanes, let us introduce into the alimentary canal some substance with which they may form compounds similar to urea. If sarcosine or methyl glycocine be such a substance we should have as the result of the combination—

$$\underset{\text{alcoholic carbamate or urethane}}{CO\begin{cases}OR\\NH_2\end{cases}} + \underset{\text{sarcosine}}{CH_2\begin{cases}NH.CH_3\\COOH\end{cases}} = \underset{\text{alcohol}}{OH.R} + \underset{\text{methyl hydantoic acid}}{CO\begin{cases}N.CH_3-CH_2-COOH\\NH_2\end{cases}}$$

and methyl hydantoic acid being an unstable body would be transformed into methyl hydantoin—

$$\underset{\text{methyl hydantoic acid}}{CO\begin{cases}N.CH_3-CH_2-COOH\\NH_2\end{cases}} = \underset{\text{methyl hydantoin}}{CO\begin{cases}N.CH_3-CH_2\\ \qquad\qquad\;\; |\\NH \text{———} CO\end{cases}} + H_2O$$

If then after the administration of sarcosine to a living animal, methyl hydantoin appears in the urine, this would furnish a strong argument in favour of the formation and existence of the urethanes in the system. I find that experiments have been made with sarcosine by Schultzen, Bauman and Hoppe-Seyler, Salkowski and von Mering*. Most recently by Dr J. Schiffer, who gives an excellent summary of the experiments and views of the others, together with an account of his own observations and experiments. From this paper† I take the following extracts:

"Few investigations in the range of physiological chemistry have in recent times excited so much attention as those of Schultzen on the transformation of sarcosine in the animal body. In conjunction with Leon v. Nenki (*Zeitsch. f. Biologie*, Bd. VIII.) he had previously found on giving glycocine as food the amount of urea excreted corresponded with the amount of N given....... He repeated his experiments with methyl glycocine or sarcosine. Increased secretion of urea did not take place; on the contrary two new bodies appeared in the urine, both having an analogous composition, the one compounded of sarcosine and carbamic acid, the other of sarcosine and sulphamic acid. The first of these bodies

* *Berichte der deutsch. chem. Gesell.* Bd. V.—VIII.
† *Zeitschrift für physiol. Chemie*, Bd. V. s. 267.

was identical in its constitution with methyl hydantoic acid, but was not recognized as such by Schultzen. He concluded from his investigation that the sarcosine attached to itself the carbamic acid resulting from disintegration of the albuminous bodies and which in a normal condition gives rise to the production of urea. With this hypothesis, it appeared very plausible that urea should disappear from urine containing sarcosine. He imagined therefore he had explained the mode in which urea was formed, and had thus solved one of the most important questions in physiological chemistry by the convincing proof of a carefully devised experiment."...............

"Salkowski found after administering taurine its uramido-acid sauro-carbamic acid in the urine (*Berichte*, VI. s. 744)."............

"Later E. Bauman and Hoppe-Seyler (*Berichte*, Bd. VII. s. 34) succeeded in forming methyl hydantoic acid synthetically under such conditions as might exist in the animal body. Equivalent amounts of sarcosine, potassium cyanate and ammonium sulphate were digested at a temperature of 104° F., the potassium sulphate removed by alcohol, and the baryta salt of the acid referred to obtained."

"In a similar manner Salkowski (*Berichte*, VII. s. 116) at the same time produced this acid, or rather, as it is easily decomposed, its anhydride, methyl hydantoin."

"So far everything seemed to confirm Schultzen's experiments. But when further experiments were made, essentially different results were obtained. These experiments were undertaken by E. Salkowski on the one hand and Bauman and v. Mering on the other. The results obtained by all completely demonstrate the absence of sarcosine sulphamic acid. As regards methyl hydantoic acid Salkowski (*Berichte*, VIII. s. 115) first stated that in the urine of dogs it appeared only in small quantity after the administration of sarcosine, whereas Bauman and v. Mering in their experiments on the human subject showed that after administering as much as 25 grammes of sarcosine, methyl hydantoic acid was entirely absent from the urine, and that Schultzen in his experiments could not have had this substance to deal with. At the same time they discovered a probable source of error in his experiments, viz. that in the presence of sarcosine Liebig's test for urea fails. They discovered also, as Salkowski also did, that a portion of the sarcosine appeared unchanged in the urine. In later communications Salkowski confirmed the view of Bauman and von Mering that, after the administration of sarcosine there is no appearance whatever of methyl hydantoic acid in the urine."

"There appeared to be very little left then from Schultzen's experiments. One point only still remained for investigation. As the uramido-acids are so easily converted into their anhydrides,

and as this is specially so in the case of methyl hydantoic acid and its conversion into methyl hydantoin, this latter substance might possibly exist in the urine after the internal administration of sarcosine. Salkowski has given great care and attention to this point, without however arriving at any definite proof."...............

"This then was the state of the question, when Professor Bauman informed me that methyl-hydantoin reduces sulphate of copper in an alkaline solution and asked me to make some fresh experiments with sarcosine founded upon this reaction."

Schiffer then describes the experiments which he performed to demonstrate the existence of methyl hydantoin in the urine, and thus sums up the result of his investigations:—

"Our knowledge therefore of the destination of sarcosine in the organism may be formulated as follows: By far the greatest part is excreted unchanged: a smaller portion, one-fifth to one-sixth, is transformed into the uramido-acid we have been discussing, or rather into its anhydride (methyl hydantoin), and a smaller portion is oxidized into methyl urea."

Schultzen's experiments therefore here receive some confirmation—and possibly further investigation may show how the discrepancies among the various investigations have arisen. The conditions under which the different animals have been kept may not have been the same, and we know that hippuric acid is not always excreted by an animal after the administration of benzoic acid. "When horses are kept in the stable or only lightly worked, their urine contains hippuric acid, but when they are put to hard work, it contains benzoic acid*," and we may conceive that it is possible under certain conditions for sarcosine to pass through the system unchanged, and under other conditions to combine with the molecule $CONH_2$ and appear in the urine as methyl hydantoin. That this takes place to some extent Schiffer's experiments prove, and that it results from the combination of sarcosine instead of NH_3 with an alcoholic carbamate appears to be the most reasonable explanation. These experiments then it seems to me are a strong proof that in the living system the amido acids glycocine, leucine, &c. are transformed into the metameric urethanes or alcoholic carbamates, which may then be further decomposed on meeting with ammonia into urea and the corresponding alcohol.

* Watts, *Dict.* III. p. 156.

March 20, 1882.

Dr Campion, Vice-President, in the chair.

Dr G. M. Bacon, M.A., was balloted for and duly elected a Fellow of the Society.

(1) *On the use of large telescopes in twilight.* By Dr J. B. Pearson.

In his paper on this subject, read on March 6th last, Lord Rayleigh stating the generally accepted law, that the brightness of an object could not be increased by the use of a telescope, referred to a recorded phenomenon apparently not in agreement with it. I offer some remarks which perhaps may help to explain that phenomenon.

With regard to a lens the law is laid down as follows in Coddington's *Optics*, pt. I. p. 219, "When the pencils of rays proceeding from an object are received by the eye after any modification, we may naturally make comparison of its apparent brightness, with that of the object itself seen directly. Supposing then that the rays of any pencil arriving at the eye from a lens are parallel, the pencil in its original state is one which attains, at the distance of the focal length of the lens from the object, a breadth equal to that of the pupil of the eye. When the object is seen directly, the pencil proceeding from any point has, on arriving at the eye, after continually diverging, only the same breadth as the other. The quantity of light therefore coming through the lens is greater than that which comes to the naked eye in the duplicate ratio of the distance of the eye from the object, to that of the lens from the same. Now as the linear dimensions of the object are magnified by the lens in the simple ratio of these distances, the surfaces are also magnified in the ratio duplicate of this, that is exactly in the same ratio as the quantity of light was found to be increased. The object therefore should appear equally bright, with or without the lens, were no light lost in passing through the glass, which however is always the case."

He continues: pt. II. p. 10, "An instrument by which a faint effect on the eye is multiplied and extended over the surface of that organ, with as little diminution as possible of its distinctness must materially assist the natural vision. Such an instrument is a telescope. The nature of this advantage will perhaps appear more clearly from the following consideration. A hundred flames of equal magnitude and brilliancy have no more essential bright-

ness than one, and yet they are visible to a greater distance because they excite more attention. In like manner a distant ship, hardly visible on the horizon at night, is easily discerned when magnified five or six times by the sailor's night glass:" and speaking of the Galilean telescope or opera-glass: "The apparent brightness of an object seen through the instrument is nearly the same as that which it presents to the naked eye, except that more or less of the light is reflected or dispersed at each surface of the glasses through which it has to pass." Speaking of the Astronomical telescope, he says that "The quantity of light condensed into each point of the image, and consequently the apparent brightness of the field, depends, *cæteris paribus*, on the aperture of the object-glass, and the transparency of the material of which it is formed," and again, p. 29, "In order to derive full benefit from the use of a telescope with any given eye-piece, the emergent pencil should just fill the pupil of the eye, in which case the apparent brightness of an object seen through the telescope would be nearly equal to that with the naked eye, as mentioned in the case of the opera-glass. The diameter of the object-glass should therefore be equal to that of the pupil of the observer's eye multiplied by the magnifying power." The same principle is laid down in Schmidt's *Optik*, s. 528 where, speaking of a refracting telescope at Dorpat with an object glass of $4\frac{1}{2}$ inches, it is calculated that, assuming the semi-diameter of the pupil of the eye to be $\frac{1}{12}$th of an inch, the clearness of the object is undiminished up to a magnifying power of 54: but that after that it diminishes rapidly, being with a magnifying power of 480 only $\frac{1}{79}$th part of what it was: and as a rule of geometrical optics, I find no exception taken to the principle itself, the younger Herschel (*Encycl. Metrop.* vol. IV. p. 396, art. *Light*) meeting the case of stars of the smallest magnitude, in the following way by saying that "While generally the apparent intrinsic brightness of the image must be less than that of the object, this supposes the object to have a sensible magnitude: but when both the object and the image are physical points, the eye judges only of absolute light, and the light of the image is therefore proportional to the apparent magnitude of the lens as seen from the object. In the case of a star for instance, the absolute light of the image is simply as the square of the aperture, and this is the reason why stars can be seen in large telescopes which are too small to be seen in small ones." I have reproduced these extracts, not so much to remove any doubts as to the law itself, as to show how clear and defined a law we have to face when we read of the phenomenon viewed at Washington, entirely confirmed as it is by what is mentioned above of "ships seen through a glass at night," and by Herschel's account, published only in 1800, of what he had noticed first at Bath in

1776: and of the certainty of which he can have had no hesitation as he says: "this observation completely refutes an objection to telescopic vision that has been drawn from what has been demonstrated by optical writers, namely that no telescope can shew an object brighter than it is to the naked eye." I will now offer two possible explanations which have suggested themselves to me of a phenomenon as to which that eminently practical observer had evidently no doubt.

One of them is based in some degree on what the younger Herschel says as to the way in which a telescope actually renders the smallest stars visible. To use a simple argument, he takes a star as a point which has no magnitude, and infers that however the small section of the sky on which the eye is for the moment concentrated is magnified by the telescope, all the augmented light received from that point through the object-glass is accumulated in one single point: which thus, though invisible to the naked, is clearly discerned by the assisted eye. It seems to me possible that the way in which the *retina* of the eye becomes susceptible of light under the circumstances referred to, is analogous to this: viz. that the eye practically receives the light only at what we may call certain star-points, on which increased brilliancy is concentrated, the rest of the *retina* remaining insensible. The wave-theory of light may assist us here. Our general language on this subject seems based on the *emission* or *corpuscular* theory: and allowing this, it is not easy to see how the quantity of light, considered originally as a luminous surface, can ever be increased, not allowing for absorption at the lenses. On the wave principle however, though the very delicate vibrations of ether received through the pupil of the eye may be unable in their simple form to affect the *retina*, still the much larger quantity of vibrations collected by a large object-glass may have this result. It is true they are spread over a proportionably larger surface of the *retina*, but being very delicate, they may produce a better effect when "spaced out" than when concentrated: so much so that the light neutralized in passing through the lenses may be more than compensated. It may be objected that the case will practically be the same on either theory, and that the rays after passing through the pupil will be as perfectly dispersed as the matter, before reaching the *retina:* but I think that without theorizing in a random manner, it is more conceivable that a stream of light would become perfectly spread out again after compression, than that separate waves or undulations of light would recover their separate course after comparative confusion in the eye-piece and pupil. When we remember that the area of the object-glass mentioned above is 720 times that of the pupil of the eye, the force of this idea becomes evident. If it is true, as is said by Coddington, that with high

magnifying powers and not very large object-glasses, the diameter of the aperture next the eye is sometimes reduced to $\frac{1}{40}$th or $\frac{1}{50}$th of an inch, the concentration of rays at this point must be enormous.

It is mainly on these last considerations, that I would suggest, secondly, that while the geometrical theory holds good for the magnification, the great accumulation of rays received through the object-glass, but then concentrated in the comparatively speaking, very narrow eye-piece, or in the pupil of the eye, may create there what may be looked on as a new centre of light, and so produce increased illumination in the eye itself. When the Sun is observed, the intense heat at that point and its power to crack the glasses is well known: and though there is formally no screen there to arrest and exhibit the light, it seems possible that the unexplained result, which certainly seems to be established, may be due in part to this cause.

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(2) *On the Rotation of a Liquid Ellipsoid about an Axis, not a Principal Axis, but lying in a Principal Plane.* By A. G. Greenhill, M.A.

In a preceding paper in Vol. IV. Part I. of the *Proceedings of the Cambridge Philosophical Society*, page 10, it was asserted that it did not appear possible for a liquid ellipsoid to rotate about an axis other than a principal axis and to have a free surface; but Mr W. M. Hicks has drawn my attention to § 6 of Riemann's article "On the motion of a liquid homogeneous ellipsoid" in the *Göttingen Transactions* for 1861, where it is asserted that a possible form of equilibrium is obtained for an axis lying in a principal plane of the ellipsoid.

Recalling the notation of the preceding paper in Vol. IV. Part. I. of the *Proc. Camb. Phil. Society*, the liquid filling the ellipsoid

$$\frac{x^2}{a^2}+\frac{y^2}{b^2}+\frac{z^2}{c^2}=1$$

is first supposed to be rotating, as if rigid or frozen, without relative displacement of its parts, about a fixed axis with constant

angular velocity whose components about the axes of the ellipsoid are denoted by ξ, η, ζ; the velocity at any point of the liquid relative to the ellipsoid is therefore zero.

Any alteration of the angular velocity will however generate motion in the liquid relative to the ellipsoid, such that if Ω_1, Ω_2, Ω_3 denote the additional component angular velocities imparted to the ellipsoid, and if U, V, W denote the component velocities of the liquid at the point (x, y, z) relative to the ellipsoid, then

$$\left.\begin{aligned} U &= \frac{2a^2}{a^2+b^2}\Omega_3 y - \frac{2a^2}{c^2+a^2}\Omega_2 z \\ V &= \frac{2b^2}{b^2+c^2}\Omega_1 z - \frac{2b^2}{a^2+b^2}\Omega_3 x \\ W &= \frac{2c^2}{c^2+a^2}\Omega_2 x - \frac{2c^2}{b^2+c^2}\Omega_1 y \end{aligned}\right\},$$

values independent of ξ, η, ζ; and then, if ω_1, ω_2, ω_3 denote the component angular velocities of the ellipsoid,

$$\omega_1 = \Omega_1 + \xi, \quad \omega_2 = \Omega_2 + \eta, \quad \omega_3 = \Omega_3 + \zeta.$$

In this manner any arbitrary motion of the liquid filling the ellipsoid, subject to the condition of uniform vorticity throughout the volume of the ellipsoid, may be supposed to have been established.

If M denote the mass of the liquid, and h_1, h_2, h_3 the components of angular momentum of the liquid about the axes of the ellipsoid, then

$$h_1 = M\frac{b^2+c^2}{5}\left\{\left(\frac{b^2-c^2}{b^2+c^2}\right)^2\Omega_1 + \xi\right\},$$

$$h_2 = M\frac{c^2+a^2}{5}\left\{\left(\frac{c^2-a^2}{c^2+a^2}\right)^2\Omega_2 + \eta\right\},$$

$$h_3 = M\frac{a^2+b^2}{5}\left\{\left(\frac{a^2-b^2}{a^2+b^2}\right)^2\Omega_3 + \zeta\right\},$$

as before; and therefore if the liquid be suddenly frozen, the component angular velocities of the ellipsoid would change to

$$\left(\frac{b^2-c^2}{b^3+c^2}\right)^2\Omega_1 + \xi,$$

$$\left(\frac{c^2-a^2}{c^2+a^2}\right)^2\Omega_2 + \eta,$$

$$\left(\frac{a^2-b^2}{a^2+b^2}\right)^2 \Omega_3+\zeta;$$

neglecting the inertia of the case which contains the liquid.

Also
$$U\frac{x}{a^2}+V\frac{y}{b^2}+W\frac{z}{c^2}=0,$$

and
$$\frac{U}{a^2}\frac{\Omega_1}{b^2+c^2}+\frac{V}{b^2}\frac{\Omega_2}{c^2+a^2}+\frac{W}{c^2}\frac{\Omega_3}{a^2+b^2}=0;$$

and therefore the relative lines of flow are the similar ellipses made by the intersection of the similar ellipsoids

$$\frac{x^2}{a^2}+\frac{y^2}{b^2}+\frac{z^2}{c^2}=\text{const.},$$

with the system of parallel planes

$$\frac{x}{a^2}\frac{\Omega_1}{b^2+c^2}+\frac{y}{b^2}\frac{\Omega_2}{c^2+a^2}+\frac{z}{c^2}\frac{\Omega_3}{a^2+b^2}=\text{const.}$$

In a state of steady motion we must have

$$\frac{d\xi}{dt}=\frac{d\eta}{dt}=\frac{d\zeta}{dt}=0,$$

leading to the conditions (from the preceding paper)

$$\frac{\Omega_1}{b^2+c^2}:\xi=\frac{\Omega_2}{c^2+a^2}:\eta=\frac{\Omega_3}{a^2+b^2}:\zeta;$$

or
$$\Omega_1=\frac{b^2+c^2}{\mu}\xi,\ \Omega_2=\frac{c^2+a^2}{\mu}\eta,\ \Omega_3=\frac{a^2+b^2}{\mu}\zeta;$$

where μ is some constant, hereafter determined.

The relative stream lines or lines of flow now lie on the parallel planes

$$\frac{\xi x}{a^2}+\frac{\eta y}{b^2}+\frac{\zeta z}{c^2}=\text{const.},$$

planes which are conjugate to (ξ, η, ζ).

For steady motion we must have in addition

$$\frac{d\omega_1}{dt}=\frac{d\omega_2}{dt}=\frac{d\omega_3}{dt}=0,$$

or
$$\frac{h_1}{\omega_1}=\frac{h_2}{\omega_2}=\frac{h_3}{\omega_3};$$

or
$$\frac{d\Omega_1}{dt}=\frac{d\Omega_2}{dt}=\frac{d\Omega_3}{dt}=0;$$

leading (by the preceding paper) to the equations

$$\mu^2-(2a^2-b^2-c^2)\,\mu+(c^2+a^2)\,(a^2+b^2)-4a^4=0,$$
$$\mu^2-(2b^2-c^2-a^2)\,\mu+(a^2+b^2)\,(b^2+c^2)-4b^4=0,$$
$$\mu^2-(2c^2-a^2-b^2)\,\mu+(b^2+c^2)\,(c^2+a^2)-4c^4=0.$$

These three equations cannot co-exist, and therefore we must put one of the three Ω_1, Ω_2, Ω_3; and therefore also one of the three ξ, η, ζ equal to zero.

Suppose $\Omega_1=0$, and therefore $\xi=0$; then

$$\mu^2-(2a^2-b^2-c^2)\,\mu+(c^2+a^2)\,(a^2+b^2)-4a^4=0.$$

Putting $\dfrac{b^2}{a^2}=x$, $\dfrac{c^2}{a^2}=y$; then the roots of this quadratic in μ are real if $(x-y)^2-8\,(x+y)+16$ is positive; and therefore if the point (x, y) lie on the shaded part of the diagram, AL being a parabola, focus S, vertex A, and $OL=4$; supposing also $b^2>c^2$, or $x>y$, which can be done without loss of generality.

Fig. 1.

The ellipsoid is now a surface of equal pressure if

$$a^2A'=b^2B'=c^2C';$$

or

$$a^2A+\frac{4c^2a^2\,(c^2-a^2)}{\mu^2}\,\eta^2-\left(\frac{c^2-a^2}{\mu}-1\right)^2a^2\eta^2$$
$$-\frac{4a^2b^2\,(a^2-b^2)}{\mu^2}\,\zeta^2-\left(\frac{a^2-b^2}{\mu}+1\right)^2a^2\zeta^2$$
$$=b^2B+\frac{4a^2b^2\,(a^2-b^2)}{\mu^2}\,\zeta^2-\left(\frac{a^2-b^2}{\mu}-1\right)^2b^2\zeta^2$$
$$=c^2C-\frac{4c^2a^2\,(c^2-a^2)}{\mu^2}\,\eta^2-\left(\frac{c^2-a^2}{\mu}+1\right)^2c^2\eta^2;$$

where

$$A=2\pi\rho\int_0^\infty \frac{abcd\lambda}{(a^2+\lambda)P},\quad B=2\pi\rho\int_0^\infty \frac{abcd\lambda}{(b^2+\lambda)P},\quad C=2\pi\rho\int_0^\infty \frac{abcd\lambda}{(c^2+\lambda)P};$$

ρ denoting the density of the liquid, and

$$P^2=(a^2+\lambda)(b^2+\lambda)(c^2+\lambda).$$

These two equations will determine η^2 and ζ^2 in terms of A, B, C, a^2, b^2, c^2; and if thrown into the form

$$Q\,\eta^2-R\,\zeta^2=b^2B-a^2A,$$

$$Q'\eta^2-R'\zeta^2=c^2C-a^2A;$$

then

$$Q=\frac{4c^2a^2(c^2-a^2)}{\mu^2}-\left(\frac{c^2-a^2}{\mu}-1\right)^2a^2,$$

$$R=\frac{8a^2b^2(a^2-b^2)}{\mu^2}+\left(\frac{a^2-b^2}{\mu}+1\right)^2a^2-\left(\frac{a^2-b^2}{\mu}-1\right)^2b^2,$$

$$Q'=\frac{8c^2a^2(c^2-a^2)}{\mu^2}+\left(\frac{c^2-a^2}{\mu}+1\right)^2c^2-\left(\frac{c^2-a^2}{\mu}-1\right)^2a^2,$$

$$R'=\frac{4a^2b^2(a^2-b^2)}{\mu^2}+\left(\frac{a^2-b^2}{\mu}+1\right)^2a^2;$$

and therefore

$$\eta^2=\frac{(b^2B-a^2A)\,R'-(c^2C-a^2A)\,R}{QR'-Q'R},$$

$$\zeta^2=\frac{(b^2B-a^2A)\,Q'-(c^2C-a^2A)\,Q}{QR'-Q'R}.$$

Captain P. A. MacMahon, R.A., has performed the algebraical reduction, which is very heavy, of these expressions, and he finds that

$$\eta^2=\mu^2\frac{a^2b^2(b^2-c^2)A-a^2b^2(4a^2-3b^2-c^2)B+c^2(a^2-b^2)(4a^2+b^2-c^2)C}{(c^2+a^2+\mu)(b^2-c^2)^2\{3a^4-(c^2-a^2)(a^2-b^2)\}},$$

$$\zeta^2=\mu^2\frac{-c^2a^2(b^2-c^2)A-b^2(c^2-a^2)(4a^2-b^2+c^2)B-c^2a^2(4a^2-b^2-3c^2)C}{(a^2+b^2+\mu)(b^2-c^2)^2\{3a^4-(c^2-a^2)(a^2-b^2)\}};$$

and substituting for A, B, and C their values

$$\frac{\eta^2}{2\pi\rho}=\mu^2\frac{(a^2-b^2)\displaystyle\int_0^\infty[(c^2-4a^2)\lambda^2-\{3a^4-(c^2-a^2)(a^2+b^2)\}\lambda]\frac{abcd\lambda}{P^3}}{(c^2+a^2+\mu)(b^2-c^2)\{3a^4-(c^2-a^2)(a^2-b^2)\}},$$

$$\frac{\zeta^2}{2\pi\rho}=\mu^2\,\frac{(c^2-a^2)\int_0^\infty\left[(b^2-4a^2)\lambda^2-\{3a^4+(c^2+a^2)(a^2-b^2)\}\,\lambda\right]\frac{abcd\lambda}{P^3}}{(a^2+b^2+\mu)\,(b^2-c^2)\,\{3a^4-(c^2-a^2)(a^2-b^2)\}}\,.$$

Since
$$\omega_2=\Omega_2+\eta=\frac{c^2+a^2+\mu}{\mu}\,\eta,$$
$$\omega_3=\Omega_3+\zeta=\frac{a^2+b^2+\mu}{\mu}\,\zeta;$$

therefore

$$\frac{\omega_2^{\,2}}{2\pi\rho}=\frac{a^2-b^2}{b^2-c^2}\cdot\frac{c^2+a^2+\mu}{3a^4-(c^2-a^2)\,(a^2-b^2)}$$
$$\int_0^\infty\left[(c^2-4a^2)\,\lambda^2-\{3a^4-(c^2-a^2)\,(a^2+b^2)\}\,\lambda\right]\frac{abcd\lambda}{P^3}\,,$$

$$\frac{\omega_3^{\,2}}{2\pi\rho}=\frac{c^2-a^2}{b^2-c^2}\cdot\frac{a^2+b^2+\mu}{3a^4-(c^2-a^2)\,(a^2-b^2)}$$
$$\int_0^\infty\left[(b^2-4a^2)\lambda^2-\{3a^4-(c^2+a^2)\,(a^2-b^2)\}\lambda\right]\frac{abcd\lambda}{P^3}\,.$$

Then the pressure is given by

$$p+\sigma\left(\frac{x^2}{a^2}+\frac{y^2}{b^2}+\frac{z^2}{c^2}\right)=\text{constant},$$

or
$$p=\varpi+\sigma\left(1-\frac{x^2}{a^2}-\frac{y^2}{b^2}-\frac{z^2}{c^2}\right),$$

where ϖ is the pressure at the surface, and

$$\sigma=\tfrac{1}{2}\rho a^2A'=\tfrac{1}{2}\rho b^2B'=\tfrac{1}{2}\rho c^2C',$$

and therefore

$$\sigma=\tfrac{1}{2}\rho a^2b^2c^2\,\frac{(b^2-c^2)\,A-(4a^2-3b^2-c^2)\,B+(4a^2-b^2-3c^2)\,C}{(b^2-c^2)\,\{3a^4-(c^2-a^2)\,(a^2-b^2)\}}$$
$$=\frac{\pi\rho^2}{D}\int_0^\infty(3\lambda^2+6a^2\lambda+D)\,\frac{a^2b^3c^3d\lambda}{P^3}$$
$$=\frac{\pi\rho^2}{D}\int_0^\infty\{3\,(a^2+\lambda)^2-(c^2-a^2)\,(a^2-b^2)\}\,\frac{a^3b^3c^3d\lambda}{P^3}\,,$$

putting
$$3a^4-(c^2-a^2)\,(a^2-b^2)=D.$$

In order that the surface may be a free surface, and therefore that ϖ may vanish, it is necessary that σ should be positive for p to be positive in the interior of the ellipsoid.

A still further reduction is required in order to determine when these values of η^2 and ζ^2 or $\omega_2{}^2$ and $\omega_3{}^2$ are positive, and to determine when σ is positive.

From $x=0$ to $x=1$, $a^2>b^2>c^2$, in the region shaded horizontally (region I.); from $x=1$ to $x=4$, $b^2>a^2>c^2$, the region shaded vertically (region II. A); from $x=1$ to $x=9$, $b^2>a^2>c^2$, the region shaded vertically (region II. B); from $x=9$ to $x=\infty$, $b^2>a^2>c^2$, the region shaded vertically (region II. B), or $b^2>c^2>a^2$, in the region shaded with slanting lines (region III.).

Riemann's condition that $b+c<2a$, or $\sqrt{x}+\sqrt{y}<2$, holds therefore within regions I. and II. A, and that $b-c>2a$, or $\sqrt{x}-\sqrt{y}>2$, within regions II. B and III.

In the reduction of A, B and C to elliptic integrals, lines of constant modulus radiate from the vertex of the parabola in the figure, AO being the line of modular angle 90°, AB of 0°, AL of 60°, AC of 90°, AS of 0°.

In order to reduce A, B, and C to elliptic integrals,

(1) in region I. we must put

$$a^2+\lambda=(a^2-c^2)\frac{1}{\sin^2\psi},$$

$$b^2+\lambda=(a^2-c^2)\frac{\Delta^2\psi}{\sin^2\psi},$$

$$c^2+\lambda=(a^2-c^2)\cot^2\psi,$$

and
$$k^2=\frac{a^2-b^2}{a^2-c^2}=\frac{1-x}{1-y}=\sin^2\theta,$$

$$k'^2=\frac{b^2-c^2}{a^2-c^2}=\frac{x-y}{1-y}=\cos^2\theta,$$

suppose, where
$$\frac{b^2}{a^2}=x,\quad \frac{c^2}{a^2}=y.$$

Then
$$A=\frac{4\pi\rho abc}{(a^2-c^2)^{\frac{3}{2}}}\int_0^\phi \sin^2\psi\,\frac{d\psi}{\Delta\psi}$$

$$=\frac{4\pi\rho\sqrt{xy}}{(1-x)\sqrt{(1-y)}}(F\phi-E\phi),$$

where
$$\sin^2\phi=1-\frac{c^2}{a^2}=1-y,$$

and therefore
$$\cos\phi=\sqrt{y},\quad \Delta\phi=\sqrt{x}.$$

Also
$$B = \frac{4\pi\rho abc}{(a^2-c^2)^{\frac{3}{2}}}\int_0^\phi \frac{\sin^2\psi\,d\psi}{\Delta^3\psi}$$

$$= \frac{4\pi\rho abc}{(a^2-c^2)^{\frac{3}{2}}}\left\{\frac{1}{k^2k'^2}(E\phi - k'^2F\phi) - \frac{\sin\phi\cos\phi}{k'^2\Delta\phi}\right\},$$

(Legendre, *Fonctions Elliptiques*, I. p. 257)

$$= \frac{4\pi\rho\sqrt{\{xy(1-y)\}}}{(1-x)(x-y)}E\phi - \frac{4\pi\rho\sqrt{xy}}{(1-x)\sqrt{(1-y)}}F\phi - \frac{4\pi\rho y}{x-y};$$

and
$$C = \frac{4\pi\rho abc}{(a^2-c^2)^{\frac{3}{2}}}\int_0^\phi \tan^2\psi\frac{d\psi}{\Delta\psi}$$

$$= \frac{4\pi\rho abc}{(a^2-c^2)^{\frac{3}{2}}}\frac{1}{k'^2}(\tan\phi\,\Delta\phi - E\phi)$$

$$= \frac{4\pi\rho\sqrt{xy}}{(x-y)\sqrt{(1-y)}}\left\{\frac{\sqrt{(1-y)}\sqrt{x}}{\sqrt{y}} - E\phi\right\}$$

$$= \frac{4\pi\rho x}{x-y} - \frac{4\pi\rho\sqrt{xy}}{(x-y)\sqrt{(1-y)}}E\phi.$$

Therefore

$$\frac{A}{4\pi\rho} = \frac{\sqrt{xy}}{(1-x)\sqrt{(1-y)}}(F\phi - E\phi),$$

$$\frac{B}{4\pi\rho} = \frac{\sqrt{\{xy(1-y)\}}}{(1-x)(x-y)}E\phi - \frac{\sqrt{(xy)}}{(1-x)\sqrt{(1-y)}}F\phi - \frac{y}{x-y},$$

$$\frac{C}{4\pi\rho} = \frac{x}{x-y} - \frac{\sqrt{(xy)}}{(x-y)\sqrt{(1-y)}}E\phi;$$

and
$$A + B + C = 4\pi\rho.$$

Therefore, in region I.,

$$x = \Delta^2\phi, \qquad y = \cos^2\phi,$$

$$\frac{A}{4\pi\rho} = \frac{\cos\phi\,\Delta\phi\,F\phi}{\sin^2\theta\sin^3\phi} - \frac{\cos\phi\,\Delta\phi\,E\phi}{\sin^2\theta\sin^3\phi},$$

$$\frac{B}{4\pi\rho} = \frac{\cos\phi\,\Delta\phi\,E\phi}{\sin^2\theta\cos^2\theta\sin^3\phi} - \frac{\cos\phi\,\Delta\phi\,F\phi}{\sin^2\theta\sin^3\phi} - \frac{\cos^2\phi}{\cos^2\theta\sin^2\phi},$$

$$\frac{C}{4\pi\rho} = \frac{\Delta^2\phi}{\cos^2\theta\sin^2\phi} - \frac{\cos\phi\,\Delta\phi\,E\phi}{\cos^2\theta\sin^3\phi}.$$

(2) in region II. $b^2 > a^2 > c^2$, and we must put

$$a^2 + \lambda = (b^2 - c^2)\frac{\Delta^2\psi}{\sin^2\psi},$$

$$b^2 + \lambda = (b^2 - c^2)\frac{1}{\sin^2\psi},$$

$$c^2 + \lambda = (b^2 - c^2)\frac{\cos^2\psi}{\sin^2\psi};$$

and
$$\frac{a^2}{b^2} = \Delta^2\phi, \quad \frac{c^2}{b^2} = \cos^2\phi,$$

or
$$x = \frac{1}{\Delta^2\phi}, \quad y = \frac{\cos^2\phi}{\Delta^2\phi},$$

and then the values of A, B, C in region II. will be the values of B, A, C respectively in region I.

(3) in region III. $b^2 > c^2 > a^2$, and we must put

$$a^2 + \lambda = (b^2 - a^2)\frac{\cos^2\psi}{\sin^2\psi},$$

$$b^2 + \lambda = (b^2 - a^2)\frac{1}{\sin^2\psi},$$

$$c^2 + \lambda = (b^2 - a^2)\frac{\Delta^2\psi}{\sin^2\psi},$$

and
$$\frac{c^2}{b^2} = \Delta^2\phi, \quad \frac{a^2}{b^2} = \cos^2\phi,$$

or
$$x = \frac{1}{\cos^2\phi}, \quad y = \frac{\Delta^2\phi}{\cos^2\phi};$$

and then the values of A, B, C in region III. will be the values of C, A, B respectively in region I.

The following tables give the values of $\frac{A}{4\pi\rho}$, $\frac{B}{4\pi\rho}$ and $\frac{C}{4\pi\rho}$, for values of θ and ϕ proceeding by increments of $\frac{1}{12}\pi$, or, expressed in degrees, for 0°, 15°, 30°, 45°, 60°, 75°, 90°; the vertical columns for constant values of θ, and the horizontal lines for constant values of ϕ.

Table of Values of $\frac{A}{4\pi\rho}$ *in region I,*

$\frac{B}{4\pi\rho}$ *in region II,*

$\frac{B}{4\pi\rho}$ *in region III.*

	0°	15°	30°	45°	60°	75°	90°
0°	·3333	·3333	·3333	·3333	·3333	·3333	·3333
15°	·3287	·3320	·3271	·3265	·3264	·3244	·3240
30°	·3138	·3128	·3099	·3055	·3009	·2972	·2959
45°	·2854	·2832	·2782	·2696	·2593	·2502	·2464
60°	·2364	·2074	·2276	·2159	·1997	·1822	·1736
75°	·1521	·1505	·1455	·1361	·1203	·0962	·0789
90°	·0000	·0000	·0000	·0000	·0000	·0000	·0000

Table of Values of $\frac{B}{4\pi\rho}$ *in region I,*

$\frac{A}{4\pi\rho}$ *in region II,*

$\frac{C}{4\pi\rho}$ *in region III.*

	0°	15°	30°	45°	60°	75°	90°
0°	·3333	·3333	·3333	·3333	·3333	·3333	·3333
15°	·3287	·3258	·3315	·3335	·3355	·3369	·3380
30°	·3138	·3160	·3221	·3314	·3412	·3495	·3521
45°	·2854	·2900	·3023	·3222	·3464	·3678	·3768
60°	·2364	·2709	·2602	·2919	·3375	·3880	·4132
75°	·1521	·1573	·1745	·2084	·2697	·3730	·4606
90°	·0000	·0000	·0000	·0000	·0000	·0000	·5000

Table of Values of $\frac{C}{4\pi\rho}$ *in region I,*

$\frac{C}{4\pi\rho}$ *in region II,*

$\frac{A}{4\pi\rho}$ *in region III.*

	0°	15°	30°	45°	60°	75°	90°
0°	·3333	·3333	·3333	·3333	·3333	·3333	·3333
15°	·3426	·3424	·3415	·3400	·3391	·3390	·3380
30°	·3724	·3712	·3679	·3631	·3579	·3534	·3521
45°	·4292	·4268	·4196	·4082	·3943	·3820	·3768
60°	·5272	·5236	·5123	·4921	·4629	·4299	·4132
75°	·6959	·6922	·6799	·6556	·6100	·5304	·4606
90°	1·0000	1·0000	1·0000	1·0000	1·0000	1·0000	·5000

Putting, with Riemann,

$$D = 3a^4 - (c^2 - a^2)(a^2 - b^2)$$
$$= 4a^4 - a^2(b^2 + c^2) + b^2c^2,$$

then we may write

$$\frac{b^2 - c^2}{a^2 - b^2}\frac{D\omega_2^{\,2}}{2\pi\rho} = (c^2 + a^2 + \mu)\int_0^\infty \frac{abc\,\lambda\,d\lambda}{P(b^2 + \lambda)}\left(\frac{b^2}{a^2 + \lambda} - \frac{4a^2 + b^2 - c^2}{c^2 + \lambda}\right),$$

$$\frac{b^2 - c^2}{c^2 - a^2}\frac{D\omega_3^{\,2}}{2\pi\rho} = (a^2 + b^2 + \mu)\int_0^\infty \frac{abc\,\lambda\,d\lambda}{P(c^2 + \lambda)}\left(\frac{c^2}{a^2 + \lambda} - \frac{4a^2 - b^2 + c^2}{b^2 + \lambda}\right),$$

and therefore Riemann's S and T are

$$S = \frac{\omega_2^{\,2}}{4abc\,(c^2 + a^2 + \mu)}, \qquad T = \frac{\omega_3^{\,2}}{4abc\,(a^2 + b^2 + \mu)}.$$

The remainder of the notation of this paper can be identified with Riemann's notation, by putting

$$p = \omega_1, \qquad q = \omega_2, \qquad r = \omega_3;$$
$$p' = \frac{2bc}{b^2 + c^2}\Omega_1, \qquad q' = \frac{2ca}{c^2 + a^2}\Omega_2, \qquad r' = \frac{2ab}{a^2 + b^2}\Omega_3;$$
$$g = h_1, \qquad h = h_2, \qquad k = h_3;$$
$$g' = -2bc\xi, \qquad h' = -2ca\eta, \qquad k' = -2ab\zeta.$$

The conditions for steady motion become

$$\frac{h_1}{\omega_1} = \frac{h_2}{\omega_2} = \frac{h_3}{\omega_2} = -\frac{E}{\mu},$$

or

$$\frac{g}{p} = \frac{h}{q} = \frac{k}{r} = -\frac{E}{\mu},$$

and

$$\frac{g'}{p'} = \frac{h'}{q'} = \frac{k'}{r'} = -\mu,$$

where

$$E = b^2c^2 + c^2a^2 + a^2b^2 - 3a^4;$$

also g, p, g', p' are zero.

Putting as before $\frac{b^2}{a^2} = x, \quad \frac{c^2}{a^2} = y;$

then

$$\omega_2^{\,2} = \frac{(y+z)\,M}{(x-y)^2\,H},$$

$$\omega_3^{\,2} = \frac{(x+z)\,N}{(x-y)^2 H},$$

where

$$M = x\,(x-y)\,A - x\,(4-3x-y)\,B + y\,(1-x)\,(4+x-y)\,C,$$
$$N = -y\,(x-y)\,A + x\,(1-y)\,(4-x+y)\,B - y\,(4-x-3y)\,C,$$
$$H = xy - x - y + 4,$$

and $z = 1 + \frac{\mu}{a^2}$,

so that z is given by the quadratic

$$z^2 - (4-x-y)\,z + xy = 0.$$

Therefore from $x = 0$ to $x = 1$ (region I.) H is positive; from $x = 1$ to $x = 4$ (region II. A) H is positive; from $x = 4$ up to the curve $H = 0$ (a hyperbola) D is negative; and beyond the curve $H = 0$, H is positive.

In regions I. and II. A, $y + z$ is positive; in regions II. B (that is, from $x = 4$ to $x = \infty$, and $y = 0$ to $y = 1$) and III. $y + z$ is negative.

In all the regions $x + z$ is always positive.

Also writing M in the form

$$M = 2\pi\rho\,(1-x)\,(x-y)\int_0^\infty \left\{(y-4)\,\frac{\lambda}{a^2} + xy - x + y - 4\right\} \frac{a^3bc\,\lambda\,d\lambda}{P^3},$$

it is seen that M is negative in region I., positive in region II., and in region III. changes sign from positive to negative in crossing a certain curve $M=0$, lying between the hyperbola

$$xy - x + y - 4 = 0$$

and the straight line $y - 4 = 0$.

Writing N in the form

$$N = 2\pi\rho\,(y-1)\,(x-y)\int_0^\infty \left\{(x-4)\,\lambda + xy + x - y - 4\right\}\frac{a^3bc\,\lambda\,d\lambda}{P^3},$$

it is seen that N is positive in regions I. and II. A, negative in region II. B, zero when $y=1$, and positive in region III.

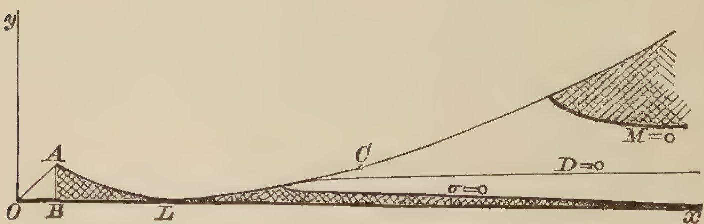
Hence ω_2^2 is negative in region I.; ω_2^2 is positive in region II. A, and in region II. B up to the curve $H=0$; ω_2^2 is negative in region II. B beyond the curve $H=0$ and in region III. up to the curve $M=0$; and ω_2^2 is positive in region III. beyond the curve $M=0$.

Also ω_3^2 is positive in regions I. and II. A, and in II. B up to the curve $H=0$; ω_3^2 is negative in region II. B beyond the curve $H=0$; and ω_3^2 is positive in region III.

Hence ω_2^2 and ω_3^2 are only both positive, and the only real solutions are contained in region II. up to the curve $H=0$ or $D=0$, and in region III. beyond the curve $M=0$.

The conditions that a free surface can exist require in addition that σ should be positive; and therefore, drawing the transcendental curve $\sigma=0$, we are restricted in region II B. to the part below the curve $\sigma=0$; and in region III. to the part above the transcendental curve $M=0$; parts represented in figure 2 by double shading.

Fig. 2.



Similarly we can draw a figure to represent to the eye, as in

figure 3, the curve which is the locus of (x, y) for Jacobi's or Dedekind's ellipsoid, in which

Fig. 3.

$$\frac{a^2A - c^2C}{a^2} = \frac{b^2B - c^2C}{b^2},$$

and $$x = \frac{b^2}{a^2}, \quad y = \frac{c^2}{a^2}.$$

Then the conditions lead to

$$\int_0^\infty \frac{\lambda d\lambda}{P^3}(a^2b^2 - b^2c^2 - c^2a^2 - c^2\lambda) = 0,$$

or $$\int_0^\infty \frac{\lambda d\lambda}{P^3}\left(x - y - xy - y\frac{\lambda}{a^2}\right) = 0;$$

and therefore the required curve must lie below the hyperbola

$$x - y - xy = 0.$$

Having plotted out the curve by means of points from $x = 0$ to $x = 1$, the points from $x = 1$ to $x = \infty$ can be drawn by putting $\frac{1}{x}$ for x, and $\frac{y}{x}$ for y; so that if

$$y = f(x)$$

is the equation of the curve from $x = 0$ to $x = 1$; then

$$y = xf\left(\frac{1}{x}\right)$$

is the equation of the curve from $x = 1$ to $x = \infty$.

There is therefore a *point saillant* on the curve when $x=1$, and then $y = \cdot 34$, and $\frac{\omega^2}{2\pi\rho} = \cdot 1871$; and the curve has the asymptote $y=1$.

As x increases from 0 to 1, y increases from 0 to $\cdot 34$, $\frac{y}{x}$ decreases from 1 to $\cdot 34$, and $\frac{\omega^2}{2\pi\rho}$ increases from 0 to $\cdot 1871$.

As x increases from 1 to ∞, y increases from $\cdot 34$ to 1, $\frac{y}{x}$ decreases from $\cdot 34$ to 0, and $\frac{\omega^2}{2\pi\rho}$ decreases from $\cdot 1871$ to 0.

Throughout the present and the preceding articles the inverse method to that of Lejeune-Dirichlet and Riemann has been employed.

These writers start by assuming the existence of surfaces of equal pressure similar to the ellipsoidal surface, and proceed to determine the motion of the liquid necessary for the existence of these surfaces of equal pressure; whereas in these articles a certain motion is supposed to have been set up in the liquid by mechanical processes, and the pressure at any point is investigated, the liquid being supposed contained in a rigid case or shell. Afterwards the conditions are investigated that are requisite for the ellipsoidal shell to be a surface of equal pressure, and that a free surface can exist.

Some algebraical errors on page 11, Vol. IV. Part I., must be corrected: line 3 should be

$$\Omega_1^{\,2} + \Omega_2^{\,2} = M + \frac{(a^2+c^2)^2}{2c^2(a^2-c^2)}\zeta^2;$$

and then

$$\left(\frac{d\zeta}{dt}\right)^2 = \frac{4c^4}{(a^2+c^2)^2}\left[LM - N^2 + \left\{L\frac{(a^2+c^2)^2}{2c^2(a^2-c^2)} - M\frac{a^2}{c^2} - N\frac{a^2+c^2}{2c^2}\right\}\zeta^2 - \frac{(a^2+c^2)^2(9a^2-c^2)}{16c^4(a^2-c^2)}\zeta^4\right],$$

and therefore ζ is an elliptic function of t, which becomes a circular function when $c^2 = 9a^2$ or $LM = N^2$.

(3) *Note on Professor Cayley's paper on the elliptic function solution of the equation* $x^3 + y^3 - 1 = 0$. By A. G. GREENHILL, M.A.

In this paper, read before the Society on May 23, 1881, Professor Cayley expresses the x and y of any point on the cubic curve

$$x^3 + y^3 - 1 = 0$$

in terms of elliptic functions of u by means of the relations

$$x = \frac{2r \operatorname{sn} u \operatorname{dn} u - (1 + \operatorname{cn} u)^2}{2r \operatorname{sn} u \operatorname{dn} n + (1 + \operatorname{cn} u)^2},$$

$$y = \frac{m(1 + \operatorname{cn} u)\{1 + r^2 + (1 - r^2) \operatorname{cn} u\}}{2r \operatorname{sn} u \operatorname{dn} u + (1 + \operatorname{cn} u)^2},$$

the modulus being $\sin 15^\circ$, and $r = \sqrt[4]{3}$, $m = \sqrt[3]{2}$.

The values of u for $x = 0$ he finds are given by the equation

$$m^6 (1 + \operatorname{cn} u)^3 = \{1 + \operatorname{cn} u + r^2 (1 - \operatorname{cn} u)\}^3,$$

the real root of which gives

$$\operatorname{cn} u = \frac{r^2 + 1 - m^2}{r^2 - 1 + m^2},$$

and therefore $u = \frac{2}{3}K$. (Legendre, t. I., § 24.)

The other values of $\operatorname{cn} u$ are obtained by writing ωm^2 and $\omega^2 m^2$ for m^2, ω being an imaginary cube root of unity.

But the corresponding values of u will be found to be *

$$\tfrac{2}{3}\omega K \text{ or } -\tfrac{1}{3}K + \tfrac{1}{3}iK',$$

and

$$\tfrac{2}{3}\omega^2 K \text{ or } -\tfrac{1}{3}K - \tfrac{1}{3}iK'.$$

For this cubic equation in $\operatorname{cn} u$ is a particular case of the more general equation

$$\frac{\operatorname{sn} u \operatorname{dn} u}{(1 + \operatorname{cn} u)^2} = \frac{\operatorname{sn} a \operatorname{dn} a}{(1 + \operatorname{cn} a)^2},$$

* *Quarterly Journal of Mathematics*, Vol. XVIII. p. 66, "On the reduction of the elliptic integrals

$$\int \frac{dz}{(z^3 - 1)\sqrt{(z^3 - b^3)}} \text{ and } \int \frac{z\,dz}{(z^3 - 1)\sqrt{(z^3 - b^3)}}."$$

or
$$\frac{(1-\operatorname{cn} u)(k'^2+k^2\operatorname{cn}^2 u)}{(1+\operatorname{cn} u)^3}=\frac{(1-\operatorname{cn} a)(k'^2+k^2\operatorname{cn}^2 a)}{(1+\operatorname{cn} a)^3},$$

or
$$1+12r^2\frac{(1-\operatorname{cn} u)(k'^2+k^2\operatorname{cn}^2 u)}{(1+\operatorname{cn} u)^3}$$
$$=1+12r^2\frac{(1-\operatorname{cn} a)(k'^2+k^2\operatorname{cn}^2 a)}{(1+\operatorname{cn} a)^3},$$

or
$$\left(1+r^2\frac{1-\operatorname{cn} u}{1+\operatorname{cn} u}\right)^3=\left(1+r^2\frac{1-\operatorname{cn} a}{1+\operatorname{cn} a}\right)^3,$$

or
$$1+r^2\frac{1-\operatorname{cn} u}{1+\operatorname{cn} u}=\omega\left(1+r^2\frac{1-\operatorname{cn} a}{1+\operatorname{cn} a}\right),$$

giving $\operatorname{cn} u=\operatorname{cn} a$, or $\operatorname{cn}\omega a$, or $\operatorname{cn}\omega^2 a$; and in the particular case considered by Professor Cayley, $a=\frac{2}{3}K$.

The equation
$$x^3+y^3=1$$
leads to the differential relation
$$\frac{dx}{(1-x^3)^{\frac{2}{3}}}+\frac{dy}{(1-y^3)^{\frac{2}{3}}}=0;$$
and to reduce to elliptic integrals, put
$$\frac{1-x^3}{(1-x)^3}=x_1^{\,3},\quad \frac{1-y^3}{(1-y)^3}=y_1^{\,3};$$
then
$$\frac{dx_1}{\sqrt{(x_1^{\,3}-b^3)}}+\frac{dy_1}{\sqrt{(y_1^{\,3}-b^3)}}=0,$$
where $b^3=\frac{1}{4}$.

Again, put
$$x_1-b=b\sqrt{3}\,\frac{1-\operatorname{cn} u}{1+\operatorname{cn} u},$$
$$y_1-b=b\sqrt{3}\,\frac{1-\operatorname{cn} v}{1+\operatorname{cn} v},$$
where the modulus $\quad k=\sin 15^\circ.$

Then
$$du+dv=0,$$
or
$$u+v=\text{constant}.$$

For the particular case when the integral is
$$x^3+y^3=1,$$

the constant is $\frac{8}{3}K$, so that we may put

$$u = \tfrac{4}{3}K - t,$$
$$v = \tfrac{4}{3}K + t.$$

Also
$$x_1^3 - b^3 = \tfrac{3}{4}\left(\frac{1+x}{1-x}\right)^2$$
$$= 3\sqrt{3}\,\frac{\operatorname{sn}^2 u \operatorname{dn}^2 u}{(1+\operatorname{cn} u)^4},$$

and therefore
$$\frac{1+x}{1-x} = 2\sqrt[4]{3}\,\frac{\operatorname{sn} u \operatorname{dn} u}{(1+\operatorname{cn} u)^2}$$

and also
$$\frac{1+y}{1-y} = 2\sqrt[4]{3}\,\frac{\operatorname{sn} v \operatorname{dn} v}{(1+\operatorname{cn} v)^2},$$

where
$$u + v = \tfrac{8}{3}K.$$

Since
$$\frac{\operatorname{sn}\frac{2}{3}K \operatorname{dn}\frac{2}{3}K}{(1+\operatorname{cn}\frac{2}{3}K)^2} = \frac{1}{2\sqrt[4]{3}},$$

therefore
$$\frac{1+x}{1-x} = \frac{\operatorname{sn} u \operatorname{dn} u}{\operatorname{sn}\frac{2}{3}K \operatorname{dn}\frac{2}{3}K}\left(\frac{1+\operatorname{cn}\frac{2}{3}K}{1+\operatorname{cn} u}\right)^2,$$
$$\frac{1+y}{1-y} = \frac{\operatorname{sn} v \operatorname{dn} v}{\operatorname{sn}\frac{2}{3}K \operatorname{dn}\frac{2}{3}K}\left(\frac{1+\operatorname{cn}\frac{2}{3}K}{1+\operatorname{cn} v}\right)^2;$$

and denoting $\operatorname{sn} u$, $\operatorname{cn} u$, $\operatorname{dn} u$ by s_1, c_1, d_1; $\operatorname{sn} v$, $\operatorname{cn} v$, $\operatorname{dn} v$ by s_2, c_2, d_2; $\operatorname{sn}\frac{2}{3}K$, $\operatorname{cn}\frac{2}{3}K$, $\operatorname{dn}\frac{2}{3}K$ by s_3, c_3, d_3 respectively;

$$x = \frac{s_1d_1(1+c_3)^2 - s_3d_3(1+c_1)^2}{s_1d_1(1+c_3)^2 + s_3d_3(1+c_1)^2},$$
$$y = \frac{s_2d_2(1+c_3)^2 - s_3d_3(1+c_2)^2}{s_2d_2(1+c_3)^2 + s_3d_3(1+c_2)^2}.$$

Since
$$u = \tfrac{4}{3}K - t, \quad v = \tfrac{4}{3}K + t;$$

therefore
$$\begin{array}{lll}
t = -2K, & x = -\infty, & y = \infty; \\
t = -\tfrac{4}{3}K, & x = -1, & y = 2^{\frac{1}{3}}; \\
t = -\tfrac{2}{3}K, & x = 0, & y = 1; \\
t = 0, & x = 2^{-\frac{1}{3}}, & y = 2^{-\frac{1}{3}}; \\
t = \tfrac{2}{3}K, & x = 1, & y = 0; \\
t = \tfrac{4}{3}K, & x = 2^{\frac{1}{3}}, & y = -1; \\
t = 2K, & x = \infty, & y = -\infty.
\end{array}$$

For other values of $u+v$ besides $\frac{8}{3}K$, the integral of the differential relation

$$\frac{dx}{(1-x^3)^{\frac{2}{3}}}+\frac{dy}{(1-y^3)^{\frac{2}{3}}}=0$$

leads to a cubic-cubic relation between x and y, which can be obtained by the elimination of t between the equations

$$\frac{1+x}{1-x}=2\sqrt[4]{3}\,\frac{\operatorname{sn} u \operatorname{dn} u}{(1+\operatorname{cn} u)^2},$$

$$\frac{1+y}{1-y}=2\sqrt[4]{3}\,\frac{\operatorname{sn} v \operatorname{dn} v}{(1+\operatorname{cn} v)^2},$$

where $u=a-t$, $v=a+t$, and $2a$ is the constant value of $u+v$.

More generally, the integral of the differential relation

$$\frac{dx}{(A+3Bx+3Cx^2+Dx^3)^{\frac{2}{3}}}+\frac{dy}{(A+3By+3Cy^2+Dy^3)^{\frac{2}{3}}}=0$$

has been obtained by Captain MacMahon, R.A., in a rational form as a cubic-cubic relation between x and y; instead of in the irrational form as left by Allégret (*Comptes Rendus*, t. 66); his result assumes the symmetrical form

$$\{A+B(x+y+z)+C(yz+zx+xy)+Dxyz\}^3=$$
$$(A+3Bx+3Cx^2+Dx^3)(A+3By+3Cy^2+Dy^3)(A+3Bz+3Cz^2+Dz^3),$$

where z is the arbitrary constant.

The general integral of

$$\frac{dx}{(1-x^3)^{\frac{2}{3}}}+\frac{dy}{(1-y^3)^{\frac{2}{3}}}=0,$$

will therefore be

$$(1-xyz)^3=(1-x^3)(1-y^3)(1-z^3);$$

or $$y^3z^3+z^3x^3+x^3y^3-3x^2y^2z^2-x^3-y^3-z^3+3xyz=0,$$

where z is the arbitrary constant; and

when $\quad z=\infty,\quad x^3+y^3-1=0;$

when $\quad z=1,\quad xy-1=0;$

when $\quad z=0,\quad \dfrac{1}{x^3}+\dfrac{1}{y^3}-1=0.$

Conversely it may be shown that the equation

$$(1-xyz)^3 = (1-x^3)(1-y^3)(1-z^3)$$

leads to the differential relation

$$\frac{dx}{(1-x^3)^{\frac{2}{3}}} + \frac{dy}{(1-y^3)^{\frac{2}{3}}} + \frac{dz}{(1-z^3)^{\frac{2}{3}}} = 0.$$

Another kind of generalization may be made, by considering the curve

$$x^3 + y^3 = 1$$

as a particular case of the curves

$$x^n + y^n = 1,$$

which give the differential relation

$$\frac{dx}{(1-x^n)^{1-\frac{1}{n}}} + \frac{dy}{(1-y^n)^{1-\frac{1}{n}}} = 0.$$

Put $$1 - x^n = \frac{w^n}{4x^n},$$

then $$(2x^n - 1)^2 = 1 - w^n,$$

or $$2x^n = 1 - \sqrt{(1-w^n)},$$

and $$2y^n = 1 + \sqrt{(1-w^n)};$$

also $$w = 2^{\frac{2}{n}} xy.$$

Then $$\frac{dx}{(1-x^n)^{1-\frac{1}{n}}} = -\frac{dy}{(1-y^n)^{1-\frac{1}{n}}} = \frac{2^{-\frac{2}{n}}\,dw}{\sqrt{(1-w^n)}};$$

and if the integrations extend from the point where

$$x = y = 2^{-\frac{1}{n}},$$

$$\int_x^{2^{-\frac{1}{n}}} \frac{dx}{(1-x^n)^{1-\frac{1}{n}}} = \int_{2^{-\frac{1}{n}}}^{y} \frac{dy}{(1-y^n)^{1-\frac{1}{n}}} = 2^{-\frac{2}{n}} \int_z^1 \frac{dw}{\sqrt{(1-w^n)}}.$$

When $n = 3$, 4, or 6, then x and y can be expressed by elliptic functions.

For our case of $n = 3$, put

$$1 - w = \sqrt{3}\,\frac{1 - \operatorname{cn} T}{1 + \operatorname{cn} T}.$$

the modulus being sin 75°; then

$$1 - w^8 = 12\sqrt{3}\frac{\mathrm{sn}^2 T\,\mathrm{dn}^2 T}{(1+\mathrm{cn}\,T)^4},$$

and

$$x^3 = \tfrac{1}{2} - 3^{\frac{3}{4}}\frac{\mathrm{sn}\,T\,\mathrm{dn}\,T}{(1+\mathrm{cn}\,T)^2},$$

$$y^3 = \tfrac{1}{2} + 3^{\frac{3}{4}}\frac{\mathrm{sn}\,T\,\mathrm{dn}\,T}{(1+\mathrm{cn}\,T)^2}.$$

By means of the cubic transformation it can be shown tha $T = t\sqrt{3}$, and if t be changed into ωt or $\omega^2 t$, then x^3 and y^3 ar unaltered, but x is changed into ωx and $\omega^2 x$, y is changed into ω and $\omega^2 y$ respectively.

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(4) *Notes on solid geometry.* By A. J. C. Allen, M.A.

The author gave a short proof of some properties of ruled sur faces, and also of the well-known theorem that if the ratio of th curvature and torsion of a curve be the same at every point, th curve is one traced on a cylinder, and cutting all the generators a a constant angle*.

May 1, 1882.

Mr F. M. Balfour, President, in the chair.

Professor Birkbeck and Mr J. E. Marr, M.A., St John's Colleg were balloted for and duly elected Fellows of the Society.

The President announced that the following resolution had bee passed by the Council: "That this Council desire to record the sense of the extraordinary value of the scientific labours of the la Charles Robert Darwin, M.A., LL.D., of Christ's College; and th a copy of this resolution be sent to Mrs Darwin as an expression the deep sympathy of the Council with herself and other membe of Mr Darwin's family for the irreparable loss they have sustained

* See *Messenger of Mathematics*, Vol. xii. pp. 26—28.

The following communications were made to the Society.

(1) *On the use of Quartz or Rock-crystal in the object-glasses of telescopes.* By Dr J. B. Pearson.

I cannot ascertain exactly how soon the rock-crystal came into use for optical purposes as distinguished from the knowledge of its optical properties. In the excellent *Encyclopædia Britannica* of 1810, I find no mention of its use for spectacles, though it is said, describing it under the head of *Mineralogy*, that "on account of its lustre and transparency it is employed in jewellery, and particularly when it is coloured, as those from Cairn-Gorm in the North of Scotland, many of which are held in high estimation," and its specific gravity is given, 2·650 to 2·888. I am inclined however to think it must have been so employed at that date, as Poggendorf, in a note to the paper I am about to refer to, in 1829, speaks of spectacles of rock-crystal as "known for a considerable time."

In the *Philosophical Transactions* for 1821 there is a short paper by Mr George Dollond on its use in Micrometers.

Dr Kitchener, writing in 1824 in his two small volumes on the Eye, where much information is given as to telescopes and their construction, simply says, with reference to spectacles, that the "notion some have as to Pebbles, that they are much cooler to the eyes than glasses," is a prejudice; and that "it is quite impossible to distinguish between good Pebbles and good Glasses."

But in Poggendorf's *Annalen*, 1829, vol. 15, pp. 244-57, there is a translation of a paper in a French serial by M. Cauchoix, the maker of the object-glass of the Northumberland Equatoreal belonging to this University, setting forth at length the advantage of substituting rock-crystal for crown-glass in the composition of achromatic object-glasses. He says that he had taken out a patent for his invention; and then goes on to describe in detail the advantages he conceives may be thus obtained. After speaking of the endeavours which had been made, especially by M. Guinand, of Neuchatel, to increase the specific gravity and so the refractive index of flint-glass, a point on which there seems to have been a good deal of controversy (see the long note at p. 246 in Poggendorf; and the paper in the *Philosophical Transactions*, by Faraday, 1830), he says he is surprised that no one had till then thought of substituting for crown-glass a substance, i.e. rock-crystal, with a higher refractive index, but a lower dispersive power; especially as in physical experiments and for micrometers (I suspect he refers to Mr Dollond's paper) he says it had already been often so employed. He does not attempt any theoretical investigations, or employ any algebraical formulæ, simply asserting that, taking as a standard, an excellent telescope by John Dollond made in 1758 of 3½ inches

aperture, and 42 inches focal length, this focal length may be re-duced to 36 inches by the use of Guinand's flint-glass instead of that of English manufacture: while he proceeds to say that the focal length, if English flint-glass be retained and combined with quartz, will be reduced to $28\frac{1}{4}$ inches and to 25 inches if Guinand's glass be employed. As an example he specifies an ordinary tele-scope of $1\frac{4}{5}$ inches aperture and 18 inches draw, which he had fur-nished with an object-glass half again as broad, the length of the telescope remaining the same.

In the *Astronomische Nachrichten* for 1836, vol. XIII., p. 273 there is a letter from M. Cauchoix, written at the request of the editor, giving some of the details I have above cited from the article in Poggendorf, and adding that he had supplied telescopes with object-glasses constructed with quartz (he does not specify the kind of flint-glass employed), which had severally (1) an aperture of six inches and a focal length of 48 inches, or in the proportion of 1 to 8; (2) $4\frac{1}{2}$ inches aperture, and focal length, 30 inches; proportion 1 to 6·6, &c.; (3) 3 inches, and 18; proportion 1 to 6; (4) $2\frac{1}{2}$ inches and 18; proportion 1 to 7; but he adds that the general proportion he would recommend is 1 to 8, or perhaps, if special excellence were desired, 1 to 10.

M. d'Abbadie, when he went on his long geodesic expedition to Abyssinia, from 1839 to 1849, during which time he returned once to visit Paris, took with him two telescopes of this construc-tion by Cauchoix, one of $75^{mm.}$ aperture and $0^{m.}90$ focal length proportion 1 to 12; the other of $72^{mm.}$ aperture and $0^{m.}84$ focal length, proportion 1 to 11·66; one of these had a small finder $25^{mm.}$ aperture and $0^{m.}15$ focal length, proportion 1 to 6, or much higher. It was the mention of these instruments by M. d'Abbadie which first drew my attention to the subject.

In 1862, M. d'Abbadie, or his editor M. Radau, seems to endorse M. Cauchoix's view as to the power of quartz to shorten a tele-scope, as he says, "l'avantage du cristal de roche est de diminuer la longeur de la lunette, longeur fort incommode en voyage." It may be that he relied on his maker, and had not examined the question for himself.

I have never seen rock-crystal mentioned anywhere else as used in the construction of object-glasses; but feeling curious on the subject, I employed Mr Hilger to make me one for an old telescope intending to compare the two object-glasses together. He has done so, but though he assures me that the radii of the lenses were cut according to the rules supplied him by a well-known gentleman whose name I do not give, but which I am sure would be perfectly satisfactory to all here present, I was much surprised to find that the crystal lens has a focal length only one half of an inch in a foot, or one twenty-fourth shorter than my old object-glass, which

by the way, as far as I can judge, is of quite average goodness, though decidedly green, as most old glasses are.

I may be allowed to say, without pretending to give an opinion of any value on a subject to which I have never given otherwise than superficial attention, that while M. Cauchoix deserves credit for a new and valuable invention, in the simple use of rock-crystal instead of crown-glass, he seems to me to be mistaken in his idea that the focal length or draw of telescopes ought to be shortened thereby. If the generally known theories on these subjects are true I should have said that the focal length of an object-glass ought practically to depend on its diameter; and I think this is generally the case; though I believe opticians generally make them as short as they well can. For example, of two telescopes, one an old one of English make of $2\frac{3}{4}$ inches object-glass, I find the proportion 1 to $15\frac{1}{2}$; of a newer French one, of $3\frac{1}{2}$ inches diameter, the proportion is 1 to 13·9. A common day telescope I possess, not new, is 1 to $13\frac{1}{2}$, i.e. not considering the erecting lenses. And if the simple and easy rules given by Sir J. Herschel in the *Edinburgh New Philosophical Magazine* for 1821 are correct, it is assumed that an achromatic object-glass may be made of any given focal length, by varying the radii of the surfaces of the lenses according to the dispersive ratio of the glasses employed; but the focal length of the compound lens is assumed to be always the same, though I should add that the proper diameter of the object-glass is not discussed by him. It is true that the table given there, p. 367, curiously enough will not be available when we use rock-crystal instead of crown-glass. Most likely, as I have said before, at that date (1821) the aim of inventors was to make flint-glass of much higher specific gravity, and so of much higher dispersive power. In the Bakerian Lecture for 1829 Faraday describes the experiments by which he, in conjunction with a committee of the Royal Society, had made a flint-glass of specific gravity 5·3, employing oxide of lead; and it must be in reference to this that it is said in the *Astron. Nachr.*, vol. VII., in the year 1829, that he had made an object-glass of $2\frac{1}{2}$ inches diameter with a flint-glass of that specific gravity, which had succeeded very well; but I am sorry to say that want of knowledge prevents me giving any farther information as to what was done in that direction. Still it would not be difficult to calculate the radii of the surfaces on Herschel's rules; and I apprehend that this is what my optician has done. It will only be necessary to extend Herschel's table, which now extends from ·50 up to ·75 dispersive ratio down to ·40, at most.

I am inclined therefore to think that M. Cauchoix's idea that he could shorten the focal length of object-glasses by using quartz, was not founded on theory, but only on some such practical experience as leads opticians to shorten telescopes when used for

terrestrial objects; and I think that the only real advantage to be gained by the use of this material is from its transparency. I am not sure how far this advantage in the present state of glass manufacture will be actually realized. Rock-crystal is undoubtedly a most beautifully transparent substance, but so is the best modern glass; and when employing alternately the old and actually greenish, though I must say well-made, object-glass, and the new one compounded with rock-crystal on objects varying in distance from half-a-mile to ten and fifteen miles, I was unable to say that one showed more illumination than the other. It is true that in an erecting eye-piece there are four lenses, in this case all of glass, so that altogether the improvement is only one-sixth instead of five-sixths (because the flint-glass must always remain in the object-glass), of that due to the actual difference in transparency between crown-glass and quartz; but I own that I expected more. At the same time I do think that a small land telescope, say of $1\frac{1}{2}$ inch object-glass, like my own, with all the lenses in the eye-piece of quartz, would be perhaps an expensive but certainly a very perfect instrument[1].

Since I read this paper, Prof. Stokes has kindly drawn my attention to the complete table of the spectrum for quartz given by Rudberg in *Poggendorf*, 1828, vol. XIV. (and also in Miller's *Mineralogy*), as well as to a paper of his own on a kindred subject in the *Proceedings* of the R. S., 1877. If we combine Fraunhofer and Rudberg's results for the three substances we are discussing, viz. flint-glass, crown-glass and quartz, and compare them with those given by Brewster, we find that while the first two experts make the dispersive powers ·068, ·039, ·032, the latter makes them ·052 (about), ·033, ·026, from which it is clear that the dispersive ratios between the first and second and the first and third are about the same, whichever authority we follow; but that the radii of the surfaces of the lenses will be considerably affected by the use of quartz, e.g. if crown-glass is used, in a compound lens of 10 inches focal length, the focal length of the flint lens ought to be about 7·63 inches, and that of the crown-glass lens 4·33 inches; while the lens compounded with quartz will require a focal length of 10·8 inches for the flint-lens, and 5·2 inches for the lens actually cut from quartz: the radii of the surfaces being correspondingly modified according to the table.

I have also obtained a copy of M. Cauchoix's patent, dated Paris, July 7, 1828. In it he simply says that rock-crystal being

[1] Herschel (*New Edinb. Phil. Mag.* VI. p. 367), stating the dispersive ratio of flint- and crown-glass to be 0·567, gives a table and rules for estimating the proper focal lengths of the *crown* and *flint* lenses for an achromatic object-glass of an assumed focal length of 10 inches. Fraunhofer's dispersive ratio is about ·577 for the same substances.

of a higher refractive index, and less dispersive than crown-glass, the substitution of the one for the other diminishes the focal length of the compound lens "when the curves of the flint lens are diminished," i.e. when the radii are increased. This seems correct, allowing that it is prudent to diminish the focal length at all. He then proceeds by claiming as original a design to substitute for flint-glass a liquid oil enclosed in glass, which is obviously only a form of Prof. Barlow's invention, which I believe has been found impracticable. Thirdly, he proposes to use rock-crystal in eye-pieces; we may say, an obvious inference from his first invention. It is clear from this short abstract of his Patent, that whatever his own method of working may have been, M. Cauchoix was offering to the public an ingenious invention rather than a scientific novelty.

For the convenience of my readers I have reprinted Fraunhofer's and Rudberg's Indices for the three substances we have been considering. The dispersive powers I have given above; or they may easily be obtained by the formula $\varpi = \frac{\mu_v - \mu_r}{\mu - 1}$, where μ, Prof. Stokes informs me, may be safely taken between D and E, about one-third of the distance from D.

	SP. G.	μB.	μC.	μD.	μE.	μF.	μG.	μH.
. Gl. *Frau.*	3·723	1·62775	1·62968	1·63504	1·64202	1·64826	1·66029	1·67106
. Gl. *Frau.*	2·535	1·52583	1·52685	1·52959	1·53301	1·53605	1·54166	1·54657
artz. *Rud.* 5·5	2·65	1·54090	1·54181	1·54418	1·54711	1·54965	1·55425	1·55817

(2) *On an altazimuth constructed from the designs of the late Rev. Dr W. Pearson.* By A. FREEMAN, M.A.

This instrument is described in Dr Pearson's *Practical Astronomy*, vol. II. pp. 464—468, and is figured in perspective on Plate XXIII. of that work. It is a portable instrument, inasmuch as it can be taken to pieces and packed in a box 31½ inches long, 16½ broad and 7 deep. It was specially designed by the late Rev. Dr Wm. Pearson, then Rector of South Kilworth, Leicestershire, and Treasurer of the Royal Astronomical Society, and was made in 1820 by Fayrer, one of Troughton's workmen. The telescope has an object-glass by Tulley with a clear aperture of 2·65 inches and a focal length of 43·4 inches: it is fitted with a direct and also with a diagonal eye-piece, and is excellent as regards defining power.

The telescope is mounted so as to be capable of reversion in altitude and azimuth, the plane of the attached circle remaining vertical during the process. The axis of motion of the vertical circle can be adjusted and retained horizontal, when the axis of motion in azimuth has been adjusted to a vertical position: these adjustments are effected by means of the levels, one of which is fixed in the plane of the vertical circle, the other hangs on the axis of that circle, the extremity of this axis remote from the circle being adjustible by opposing screws to perpendicularity with the vertical axis of the instrument.

The motion in azimuth results from the revolution of a hollow cone, about two feet long, about an upright axis having cylindrical bearings, and fitted securely to the tripod base.

The chief peculiarity of the instrument is the bracket attached to the revolving cone and supported by a prop. This bracket and the horizontal solid axis which it bears form a counterpoise to the vertical circle and the telescope.

The vertical circle is supplied with a three branch alidade which revolves about the horizontal axis to which this circle is rigidly fixed by strong radial bars. Each branch of the alidade bears a vernier read by a microscope applied successively to each. The reading of the verniers may also be effected after clamping the alidade to the circle by bringing each in succession opposite to a micro-telescope fitted to the frame of the instrument.

The three branches of the alidade are moreover connected by a

circular rim at right angles to the plane of the circle, and this rim can be clamped to the revolving cone of the azimuthal motion.

The vertical circle can be clamped either to the revolving cone, or to the alidade.

The telescope is capable of adjustment so that its line of collimation may be parallel, or may make a small fixed angle with the axis of the revolving cone, by means of a microscope fixed to that cone and viewing on its cross wires a small dot on a slightly adjustible piece of platinum borne by a short arm projecting from the tube of the telescope.

This instrument, after the death of the designer, passed into the hands of a country gentleman, who sold it to the Rev. N. S. Godfrey, Vicar of St Bartholomew, Southsea, from whom I purchased it about Easter 1881. With the sanction of the Museums Syndicate I have added it by gift to the Plumian Professor's collection of apparatus, having found it to be a very useful example of a well graduated and adjustible altazimuth telescope, or zenith instrument.

The *vertical circle* is graduated to every $5'$ of arc and is read by three verniers to every $5''$, its radius is about 8 inches.

The *horizontal circle* is graduated to every $10'$ of arc and is read by two verniers to every $10''$, its radius is about 6 inches.

I have determined the errors of excentricity of both alidades in magnitude and direction, as also the angles between their arms by discussion of the readings of each circle in three or more positions, representing each reading in the form given in Chauvenet's *Astronomy*, vol. II. § 27.

These errors are small. The *magnitudes* of the errors of excentricity are $8''{\cdot}487$ for the vertical circle, and $14''{\cdot}046$ for the horizontal circle, estimated in arcs of the respective graduated circles, equivalent respectively to $\frac{33}{10,000}$ and $\frac{41}{10,000}$ of an inch. It is unnecessary to state the direction of the error, since if all the verniers be read, the means of the readings in two different positions of the instrument determine *without error* the angles through which the circles have been turned from the one position to the other.

The value of a scale-division of the striding-level is $1''{\cdot}12$, that of a scale-division of the attached-level is $2''{\cdot}54$.

In the focus of the object-glass is a frame containing one horizontal and five vertical threads. The intervals of the vertical threads were determined by collimating each in succession on the image of the fixed cross-wires of an opposed auxiliary telescope, noting for each coincidence the readings of one of the verniers of the horizontal circle of the instrument. The intervals being small, it was unnecessary to read both verniers. In succession from the apparent left of the field of view, i.e. from the side nearest the revolving cone of the instrument, the intervals were $6'\ 40''$, $5'\ 40''$,

6′ 30″, 6′ 30″. These threads therefore have been very improperly placed by the opticians (Troughton and Simms) whom I employed for that purpose. The original threads were removed by Mr Godfrey. Inequality of intervals might however usefully remove doubts as the position of the instrument when it was employed, whether to the right or to the left of its axis.

The magnifying powers of the eye-pieces are only moderate.

The illumination of the field of view is capable of improvement.

A cylindrical zinc cover has recently been made, in two parts fitting each other, so as to protect the instrument from weather, when adjusted on a stone base provided for it on the isolated wall of the Plumian Professor's Observatory.

May 15, 1882.

Mr F. M. Balfour, President, in the chair.

The President announced that the adjudicators for the Hopkins Prize for the period 1871—73 had awarded the prize to Lord Rayleigh for his various important papers connected with the Theory of Vibrations, and particularly for his paper on the Theory of Resonance.

The following communications were made to the Society:

(1) *On the measurements of a bead of platinum, by the late Professor W. H. Miller.* By Professor W. J. Lewis.

[Introduction. Some time ago Mrs Miller entrusted to me the following work of the late Professor Miller, with a statement that he was at work on it at the period when his health finally broke down. The actual measurements were, I believe, all made during the autumn of 1874, and I remember Prof. Miller showing me the arrangement of the goniometers when I came to study with him in September of that year. There is no note of the exact manner in which the upper goniometer was fixed and adjusted. If the method be again resorted to, the neatest way of arranging the goniometers would be to clamp to the horizontal

graduated circle a disc of brass having three slots, or one slot and a three faced hole in it, in which the levelling screws of the vertical goniometer would rest. The work was left in an incomplete state, and only notes for the paper, the table of measurements and a somewhat rough stereographic projection were found. I have thrown these notes into a slightly more complete form and have made an accurate stereographic projection (see Plate) on a larger scale. In this projection the centre gives the zero of the horizontal circle, and the distance of any point from the centre gives the reading of this circle. The zero of the vertical circle is at the top of the projection, and the readings of this circle proceed from this point round to the left. Poles at a distance from the centre exceeding 90° fall below the paper and would truly be situated outside the primitive circle. By supposing the position of the eye changed to the extremity of the diameter perpendicular to the primitive *above* the paper, these poles will fall within the primitive; and their positions on this supposition are shown by surrounding the pole by a circlet. It will be noticed that poles 43 and 145, 55 and 140, 57 and 139, and 67 and 135, are very nearly above one another. They would be accurately superposed if the plane of projection were a plane of symmetry with respect to these poles. Prof. Miller has left no note of the way in which he actually made the observation; but it is clear that he had considerable confidence in the accuracy of the observations, and that the two goniometers were well adjusted. His usual plan was to bring the image of a bright signal (a minute triangle or line) reflected from each face of a zone into coincidence with that of a faintly illuminated vertical line as seen in a vertical blackened mirror. The readings of the two circles for any face are those given when the image of the bright signal was superposed on that portion of the faint line which lay in the horizontal plane through the bright signal. There are thirty-nine poles below the paper none of which are parallel to those above. This would be manifest at once during the observations, for such planes would be successively adjusted by rotating the vertical circle alone through 180°. Testing the zonal relations by means of the stereographic projections I have only succeeded, with such trial as I have given it, in finding about four or five poles to be common to any one zone. Were the bead a simple or twin crystal belonging to the cubic system (that accepted for platinum) it is almost certain that a much larger number of planes would lie in zones. I give such zones as I have observed by inclosing the numbers of the poles in square brackets, and append a query (?) whenever the pole does not accurately lie in, but is very near to, a zone; [94, 90, 51, 52, 141], [96, 67, 76, 75, 138, 88 (?)], [52, 30, 22 (?), 26, 71, 123], [59, 30, 43, 73 (?)], [42, 30, 33, 54 (?)], [94, 75, 78 (?), 124, 125], [140,

85, 44 (?), 39, 59 (?)]. The angles (30, 42) = 27° . 44′½ ; (30, 33) = 20° . 51′ ; (33, 54) = 28° . 4′⅓ have been calculated. Assuming them to be in a zone, the anharmonic ratio was found to =1. Other angles were determined approximately from the stereographic projection, but none of those found seemed to be near those between the common planes of cubic crystals: (71) and (69) are nearly at 90° to (30), as also the planes (119) and (136) below the paper.

The following are Professor Miller's notes which I have done no more than edit.

W. J. LEWIS.]

In 1874, I received from Major [now Colonel] Ross what appeared to be a bead of platinum, of approximate diameter 0·73 mm., which he had fused with the aid of a blowpipe, and which on cooling exhibited a large number of crystal-faces. The measurement of the crystal was undertaken under the impression that it would be comparatively easy, but the small size of the faces, which are not arranged in zones, rendered their identification all but impossible. The following method of determining their position was therefore adopted. The bead was attached to the axis of a small Wollaston's goniometer in a convenient position but without any special orientation. The small goniometer was then secured in an upright position on the graduated horizontal plate of the large goniometer so that the crystal lay in the intersection of their axes. The position of all the faces, which were not obscured by the vertical circle, could now be determined, by reference to the circles of the two goniometers, in the same way that the positions of stars can be referred to two planes by means of an altazimuth. In this way 147 faces, occupying somewhat more than a hemisphere were determined, and a stereographic projection of their poles made, in which the centre represents the zero of the horizontal circle, whilst the zero of the vertical circle is placed at the top of the projection and the angles read towards the left. As far as examined, the bead seems to differ from a crystal, (1) in that the planes do not seem to occur in pairs of parallel ones, (2) in that they are not arranged in zones. The relations of the planes to one another cannot therefore be very simple.

No. of face.	Horizontal circle.	Vertical circle.	No. of face.	Horizontal circle.	Vertical circle.	No. of face.	Horizontal circle.	Vertical circle.
	° ′	° ′		° ′	° ′		° ′	° ′
1	19·52	302·38	50	58·29	228·30	99	87· 1	280· 5
2	20·15	311·39	51	61· 3	202·48	100	87·24	42·54
3	21·41	149·26	52	61·11	163· 5	101	88·24	138·45
4	23·36	36·30	53	62· 5	242·11	102	88·24	176·33
5	24·42	70·10	54	62· 5	246·20	103	88·24	356· 0
6	25· 1	274·14	55	62· 5	139·40	104	89·28	89·38
7	28·39	274·16	56	62·28	86·13	105	89·28	320·27
8	29·26	306·57	57	64·31	97·50	106	89·40	346·22
9	29·31	26·22	58	65· 5	284·41	107	90· 0	291· 7
10	29·41	5· 0	59	65·25	310·51	108	90· 1	190· 6
11	31·35	213·30	60	65·25	47·22	109	90·54	209·21
12	31·51	107·13	61	66·52	23·43	110	91·40	203· 5
13	32·31	149·12	62	67·32	72·32	111	92·40	151·16
14	33· 3	309·10	63	67·36	210·48	112	94·25	299·24
15	35·35	184·18	64	67·49	181·11	113	94·54	103·18
16	37·30	215·41	65	68· 3	266·46	114	95· 0	34· 6
17	38· 8	232·24	66	68· 3	16·35	115	96·12	325·26
18	38·35	326·12	67	68· 3	150· 5	116	96·40	169·37
19	39·14	358·30	68	68·19	222·27	117	96·50	41·28
20	39·24	36·25	69	69· 9	75·53	118	96·50	241·20
21	39·44	36·25	70	70· 3	60·48	119	98·41	269·51
22	40·12	223·50	71	70· 3	297·33	120	99·16	341·40
23	41·16	273· 0	72	71· 1	143·27	121	99·53	224·27
24	42·47	148·22	73	71·15	162· 0	122	99·53	2·26
25	43·56	162·19	74	72·26	182·28	123	100·48	125·18
26	43·56	255·55	75	72·26	314·58	124	102·28	83·10
27	44·46	289· 7	76	72·38	134·55	125	102·28	184·30
28	44·53	314· 2	77	72·38	182·32	126	102·28	356·44
29	44·53	90·37	78	72·51	326·10	127	103·45	55·20
30	46·14	187·49	79	73·50	316·40	128	105· 6	4·13
31	46·38	113·22	80	74·40	140·45	129	105· 6	22·20
32	47·47	238·57	81	74·40	294·26	130	105·21	296· 9
33	48·51	216· 0	82	75·13	31·38	131	105·45	37·21
34	49·10	2·39	83	76· 9	340·26	132	105·45	113·48
35	50·13	67·13	84	76·36	49·50	133	110·23	102· 0
36	51· 0	275·17	85	76·51	86·43	134	111·10	163·24
37	51· 0	20·10	86	77·42	169·10	135	112·15	150·17
38	52·52	96·33	87	77·50	101·45	136	112·56	121·27
39	52·53	45· 6	88	77·50	122·38	137	114·37	352· 2
40	53·11	214·15	89	78· 4	197·43	138	114·51	338·56
41	53·33	287· 2	90	78·11	252·12	139	115·11	98·22
42	53·46	152·39	91	78·26	290·45	140	119· 1	140·38
43	54·30	175·18	92	78·42	236·20	141	120·13	9· 3
44	56·20	49·32	93	79·17	212· 3	142	120·48	51·49
45	57·25	188·22	94	80·26	256·11	143	120·58	19·15
46	57·26	297·16	95	82·57	156·44	144	123·11	37·28
47	57·30	33·47	96	83·32	265·25	145	124·41	176·36
48	58· 0	333·30	97	85· 5	199·55	146	125· 7	62·37
49	58·11	128·19	98	86·15	128·13	147	125·19	80·35

(2) *On a crystal of Stephanite from Wheal Newton.* By Prof. W. J. Lewis.

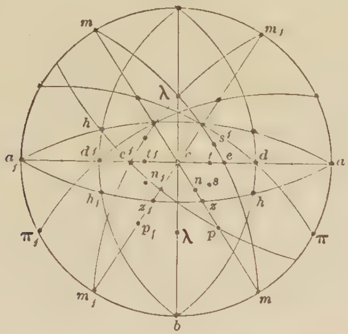
Last summer Prof. Warrington Smyth was good enough to lend me for examination a remarkably fine crystal of stephanite, which he had obtained some years previously from Wheal Newton in Cornwall, and which is, I believe, the only specimen so far known from an English locality. It is now placed in the Jermyn Street Museum. It is implanted on a large fragment of matrix of chalybite and quartz about four inches long by about two and a half wide. The specimen was fixed for the observations in the apparatus described by Prof. Miller in a paper on his goniometer in the *Phil. Magazine*, 5th series, vol. II., p. 281, 1875. From the size of the matrix, and the weight of the specimen, considerable difficulty was experienced in the accurate adjustment of the zones, and no great attention was paid to centreing the edges when once the zone-axis was placed vertical. Most of the faces gave good definite reflexions; but various artifices had to be resorted to for the purpose of obtaining some of the measurements. I was quickly struck with the smallness of the errors in the angles observed when compared with the angles given in Miller's *Mineralogy*. These errors are mainly due to two causes: (1) the error arising from inaccurate coincidence of the two signals, (2) that due to the eccentricity of the crystal. The first was probably, from the difficulties of the observations, at times considerable, and seemed likely to be sufficient to account for the whole error. It would also vary greatly in the measurement of different angles as the matrix would come more in the way at one time than at another. The latter I had not up to that time carefully estimated, but it seemed capable of approximate determination. I have thought the examination of this error of sufficient interest to append to this paper, and the more so as it is an error which has called forth a good deal of criticism in Germany. Schrauf in his *Lehrbuch der Phys. Mineralogie*, vol. I. p. 210, has given an expression for it which gives quite an erroneous idea, and seems applicable only to an utterly impossible method of observation.

The forms, and the relation of the principal zones, observed on the crystal are shown on the stereographic projection (fig. 1). The crystal is a combination of the forms

$$a(100),\ b(010),\ m(110),\ \pi(310),\ p(111),\ z(112),\ n(113),\ c(001),$$
$$t(203),\ e(101),\ d(201),\ h(412),\ s(314),\ \text{and}\ \lambda(011).$$

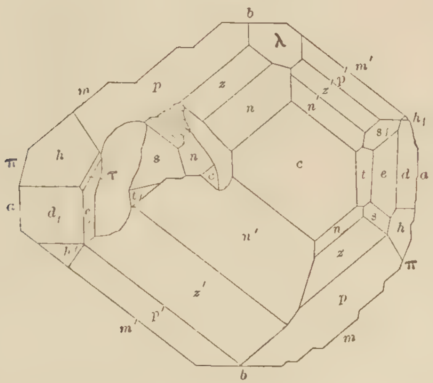
The crystal has a peculiar hemimorphic development due to the preponderance of one set of the planes p, z, n over the others of

(fig. 1).



the same forms. This is to a certain extent shown in the diagram (fig. 2), which is a projection of the crystal on the plane c. The

(fig. 2).



crystal is, however, not so wide across in comparison to its length as is shown in this diagram. The crystal is, moreover, much traversed by twin laminæ and twin striations, which are not similarly developed on like planes, or even on all parts of one and the same plane. Thus the two planes p are finely and regularly striated, as shown in the diagram, except in a small portion of one of them adjoining the plane λ. The large plane p' has a few striations, or rather thin laminæ traversing it, perpendicular to the edge $[p'\ m']$. A well-marked lamina traverses the small planes p', z', n'. Few

of the planes are free from interruptions, which seem to be due to narrow twin laminæ. Of the planes in the prism-zone one, *m*, lying under the largely-developed *p*, *z*, *n* planes is smooth; the others are all striated in a vertical direction, and seem to alternate frequently with one another, giving the more crowded end of the crystal a somewhat rounded appearance. The dome λ (011) is well developed on one side only, it appears on the other side in minute triangles lying on the edges [*n′ p*] and [*z′ p*] where these edges are interrupted by twin lamination. The large λ plane is very rough and is covered with a number of rounded bosses rising above the general level of the plane. It was impossible to obtain any reflexion better than a faint illumination from this face. The reflexions from most of the pyramid and dome planes were good and definite. The crystal is slightly broken at the two places marked in the diagram. The following table gives the angles observed and those calculated from Miller's elements:

	Calculated	Observed
cn	23°·14′	23°·10½′
nz	9·32	9·32¼
zp	19·24	19·24¼
pm	37·50	37·55½
ct	24·33¾	24·33½
ce	34·26	34·28¼
cd	53·54	53·46½
ca	90·0	90·25 (approx.)
cs	30·11	30·8½
hr	33·32	33·35¾
zz′	33·30	33·32
z′h′	33·32	33·34
h′a′	39·43	39·45½ (approx.)
πd	44·26½	45·15 "
dz′	43·15½	43·11
z′p	45·30⅔	45·32¼
ph	29·18⅓	29·15
ph	62·23	62·22¾
pd	45·28½	45·23½
pm′	109·59½	109·59
es	15·23¼	15·17
sz	31·4½	30·56
zλ	56·6½	56½ (approx.)
zm′	76·27	76·27

pn'	45·41½	45·49⅔
$n'e'$	28·45½	28·43
$e'\tau(\bar{16},\bar{3},10)$	18·2½	18·25 or
$e'\tau(\bar{21},\bar{4},13)$	18·24½	18·36½
τh		7·30
$e'h$	26·8½	26·6
$m'd'$	64·31	64·37
$m'\tau(\bar{16},\bar{3},10)$	78·19½	78·41 or
$m'\tau(\bar{21},\bar{4},13)$	78·27	79·4 (best)

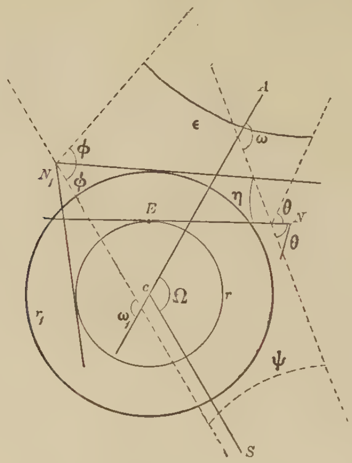
The plane τ is a narrow uneven plane, and gives two images, neither of which seem to be accurately in the zone $[m'd']$. Its indices, if it be taken to be truly in this zone, will be $(\bar{5}\,1\,3)$, a form given by Schröder. This plane requires $\tau e' = 19^{\circ}{\cdot}35\frac{1}{2}'$, which is 1° too great, a difference which cannot be accounted for by the errors of observation. No nearer approximation can be made without introducing high indices. The nearest planes in the zone $[ph'e']$ are $(\bar{16}, 3, 10)$ or $(\bar{21}, \bar{4}, 13)$. The former plane gives $\tau e'$ $18^{\circ}{\cdot}2\frac{1}{2}'$, the difference between which and the observed value is hardly within the possible errors of observation. The latter plane gives $\tau e'$ $18^{\circ}{\cdot}24\frac{1}{2}'$, and the only objection is the magnitude of the indices. I have given the angles which these two planes make with m'; angles which, judging from the close agreement between most of the observed and calculated angles, do not agree satisfactorily with either of the values of $m'\tau$ observed. τ lies on the edge of the portion where the crystal has been injured by a tool, and may possibly have suffered a slight strain from the blow.

Expression for the error due to eccentricity.

Let C in fig. 3 be the position of the axis, and let the signals be at distances a and b along the lines AC and SC. (These distances are about ten metres for the instruments at Cambridge.) Let $ACS = \Omega$; an angle which can be determined by observation with sufficient accuracy. Suppose the edge to be at N and N_1 when the observation is made for each face, and CE the normal to the first face in its first position. Then α, the angle through which the faces or normals are turned, is also the angle NCN_1. Let η be the difference between this angle and α_1 that between the two faces. The sign of η can be readily determined by considering the nature of the inequality between the angle of incidence and that made by the line of sight with the second face, when it is brought into parallelism with the first. In the case supposed in the diagram it is subtractive. Let $\frac{\pi}{2} - \theta$, $\frac{\pi}{2} - \phi$ be the angles of incidence on

the two faces, and ω and ω_1 the angles which the reflected rays make with AC. Let ϵ and ψ be the angles (in circular measure)

(fig. 3).



which NN_1 subtends at the bright and faint signals respectively; and let the projections of the broken line NCN_1 perpendicular to AC and SC be σ and τ. Then $\epsilon = \frac{\sigma}{a}$, and $\psi = \frac{\tau}{b}$. It is clear these angles will have their greatest values if the parts of the faces used be those immediately at the edge. This will also be the only position in which the faint signal can be distinctly seen behind the crystal. I shall therefore suppose the effective parts to be at the edge at N and N_1. Now

$$CN^2 \sin^2 \alpha_1 = r^2 + r_1^2 - 2rr_1 \cos \alpha_1;$$

where r and r_1 are the radii of the two circles enveloped by the faces. Also, if

$$NCE = \gamma, \quad \tan \gamma = \frac{EN}{CE} = \frac{r_1 - r \cos \alpha_1}{r \sin \alpha_1} = \frac{r_1}{r} \operatorname{cosec} \alpha_1 - \cot \alpha_1;$$

where α can be introduced to give an approximate value of γ.

From the diagram we see that

$$ECA = \frac{\pi}{2} - \overline{\omega - \theta},$$

therefore
$$NCA = \gamma - \left(\frac{\pi}{2} - \overline{\omega - \theta}\right).$$

Also
$$NCN_1 = NCA + N_1CA.$$

Again $$SCN = \Omega - NCA,$$
and $$SCN_1 + SCN = 2\pi - NCN_1.$$
Hence
$$\sigma = CN(\sin NCA + \sin N_1CA)$$
$$= 2CN \sin \frac{NCA + N_1CA}{2} \cos \frac{N_1CA - NCA}{2}$$
$$= 2CN \sin \frac{\alpha}{2} \cos \left(\frac{\pi}{2} - \overline{\omega - \theta} - \gamma + \frac{\alpha}{2}\right);$$
therefore $$\epsilon = \frac{\sigma}{a} = 2\frac{CN}{a} \sin \frac{\alpha}{2} \sin \left(\overline{\omega - \theta} + \gamma - \frac{\alpha}{2}\right).$$
Similarly
$$\tau = 2CN \sin \frac{\alpha}{2} \cos \frac{SCN - SCN_1}{2}$$
$$= 2CN \sin \frac{\alpha}{2} \sin \left(\Omega - \overline{\omega - \theta} - \gamma + \frac{\alpha}{2}\right);$$
therefore $$\psi = \frac{\tau}{b} = 2\frac{CN}{b} \sin \frac{\alpha}{2} \sin \left(\Omega - \overline{\omega - \theta} - \gamma + \frac{\alpha}{2}\right).$$
Now if the signals be at a considerable distance in comparison with the radii of the circles, the differences $\Omega - \omega$, $\Omega - \omega_1$, $\omega - 2\theta$, $\omega_1 - 2\phi$, will all be infinitesimals of the first order; and $\omega - \theta$ may be replaced in the above formulæ by $\frac{\Omega}{2}$. The two angles ϵ and ψ can now be found.

Again $\psi = \omega - \omega_1$, $NAC = 2\theta - \omega$ and $N_1AC = \omega_1 - 2\phi$;
therefore $$\epsilon = 2(\theta - \phi) + \omega_1 - \omega.$$
But
$$\eta = \overline{\omega_1 - \phi} - \overline{\omega - \theta} = \overline{\omega_1 - \omega} + \overline{\theta - \phi} = \frac{\epsilon - \psi}{2}.$$

Hence the value for the correction of the observed angle can never exceed half the angle subtended by the positions of the edge at either signal.

May 29, 1882.

PROFESSOR BABINGTON, VICE-PRESIDENT, IN THE CHAIR.

(1) *Note on a table showing the time and place of the transit of any star across the prime vertical circle in latitude* 52° 12′ 10″. By A. FREEMAN, M.A.

Several problems of Practical Astronomy may be well solved by the employment of a well-contrived altazimuth telescope so placed

that its line of collimation describes the plane of the prime vertical circle, that is, moves so as to be always in the vertical plane at right angles to the meridian. It is an additional advantage if the horizontal axis of the instrument be easily reversible on the fixed north and south supports of its pivots.

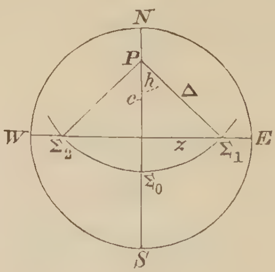
Transits of the same star across the east and west portions of the prime vertical are to be observed; the latitude may then be inferred if the declination be known; or the declination and right ascension of unregistered stars may be found from noting the times of their transits, if the latitude has been already determined from the transits of known stars.

If the vertical circle be large and well graduated, and if a micrometer eye-piece be employed, observations of the zenith distances of stars at their east and west passages will show the effects of refraction. For the true zenith distances may be calculated if we know two of the three quantities, *latitude, declination, hour-angle of transit;* and with these the observed zenith distances may be compared.

Preliminary to a series of such observations it is productive of great saving of time to tabulate at close intervals of north polar distance the corresponding hour-angles and zenith distances with their corresponding mean refractions at transit over the east or west portions of the vertical circle.

We are thus enabled to set the instrument sufficiently near to its proper position to find the star in the field of view at a sidereal time determined beforehand from the known R. A. of the star and the hour-angle interpolated from the table.

The formulæ on which the table depends are extremely simple. If Δ be the polar distance of the star, h its hour-angle from the meridian at transit over the prime vertical, z the zenith distance at the same instant, and c the complement of the latitude, we have



$$\cos z = \frac{\cos \Delta}{\cos c} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (1),$$

and

$$\cos h = \frac{\tan c}{\tan \Delta} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (2).$$

From these, with a value of $c = 37° 47' 50''$, which corresponds nearly to the place of the transit instrument at the New Museums, all that part of the table (except the refractions) which corresponds to polar distances ranging from 50° to 90° at intervals of 30′ was computed at my request by one of the sons of Mr Todd, the Junior Assistant at the Observatory.

The earlier part of the table, computed by myself, ranges from 38° to 45° of polar distances at intervals of 15′, and then from 45° to 50° at intervals of 20′. To obtain the desired quantities with sufficient accuracy a modification of the formulæ was requisite. We find from those already given by introducing the expressions for $\frac{1-\cos z}{1+\cos z}$ and $\frac{1-\cos h}{1+\cos h}$ the formulæ

$$\tan^2 \frac{z}{2} = \tan \frac{\Delta + c}{2} \cdot \tan \frac{\Delta - c}{2} \quad \text{.................. (3)},$$

$$\tan^2 \frac{h}{2} = \frac{\sin (\Delta - c)}{\sin (\Delta + c)} \quad \text{.............................. (4)}.$$

When Δ is not greatly different from c, these formulæ are absolutely necessary, and in most cases they would be more exact for logarithmic computation until Δ approaches the value 90°. The original formulæ are however sufficiently exact in the major part of the table, as is evident from the steady progression of the differences both of the hour-angle and of the zenith distances.

The refractions are obtained in about the last half of the table by successive approximations. This is necessary, inasmuch as the tables of refraction ordinarily accessible, give the refraction corresponding to an observed zenith distance, whereas an object is to assign the refraction corresponding to a true zenith distance in order to derive from it the apparent, or to-be-observed, distance.

I find since that de Zach has suggested the method of approximation which it occurred to me to employ. See his Introduction to his Tables of Bessel's Mean Refractions printed in the *Correspondance Astronomique du Baron de Zach*, vol. IX., p. 116. Génes, 1823*.

For the convenience of students of astronomy resident at Cambridge, I have caused the following table for transits over the prime vertical to be printed. A copy will be given to any one who desires it.

* Professor Adams has pointed out to me that Bessel in his Essay *Einfluss der Strahlenbrechnung auf Mikrometer-beobachtungen* (Astronomische Untersuchungen, Vol. I. p. 153) has given a method of determining the refractions corresponding to true zenith distances, and has also constructed a table of such refractions (p. 198).

Table showing the time and place of the Transit of any Star across the prime vertical circle in latitude 52° 12′ 10″.

Polar Distance.	Hour-Angle.	Difference.	Zenith Distance.	Difference.	Refraction.
° ′	h. m. s.	m. s.	° ′ ″	° ′ ″	″
38 0	0 27 39·7		4 15 17		4·4
		13 41·0		2 6 30	
38 15	0 41 20·7		6 21 47		6·5
		10 16·5		1 34 38	
38 30	0 51 37·2		7 56 25		8·1
		7 58·6		1 19 15	
38 45	0 59 35·8		9 15 40		9·5
		7 9·3		1 9 43	
39 0	1 6 45·1		10 25 23		10·7
		6 34·1		1 3 4	
39 15	1 13 19 2		11 28 27		11·8
		5 52·8		58 7	
39 30	1 19 12·0		12 26 34		12·9
		5 27·0		54 13	
39 45	1 24 39·0		13 20 47		13·8
		5 4·9		51 9	
40 0	1 29 43·9		14 11 56		14·7
		4 46·5		48 32	
40 15	1 34 30·4		15 0 28		15·7
		4 30·6		46 19	
40 30	1 39 1·0		15 46 47		16·5
		4 16·9		44 26	
40 45	1 43 17·9		16 31 13		17·3
		4 5·0		42 48	
41 0	1 47 22·9		17 14 1		18·1
		3 54·3		41 19	
41 15	1 51 17·2		17 55 20		18·8
		3 44·7		40 4	
41 30	1 55 1·9		18 35 24		19·6
		3 37·1		38 54	
41 45	1 58 38·0		19 14 18		20·4
		3 28·4		37 52	
42 0	2 2 6·4		19 52 10		21·1
		3 21·1		36 55	
42 15	2 5 27·5		20 29 5		21·8
		3 14·6		36 3	
42 30	2 8 42·1		21 5 8		22·5
		3 8·5		35 16	
42 45	2 11 50·6		21 40 24		23·2
		3 2·9		34 36	
43 0	2 14 53·5		22 14 0		23·9
		2 57·7		33 48	
43 15	2 17 51·2		22 48 48		24·5
		2 52·8		33 12	
43 30	2 20 44·0		23 22 0		25·1
		2 48·2		32 42	
43 45	2 23 32·2		23 54 42		25·8
		2 43·9		32 7	
44 0	2 26 16·1		24 26 49		26·5
		2 40·0		31 37	
44 15	2 28 56·1		24 58 26		27·2
		2 36·0		31 9	
44 30	2 31 32·1		25 29 35		27·8
		2 32·5		30 42	

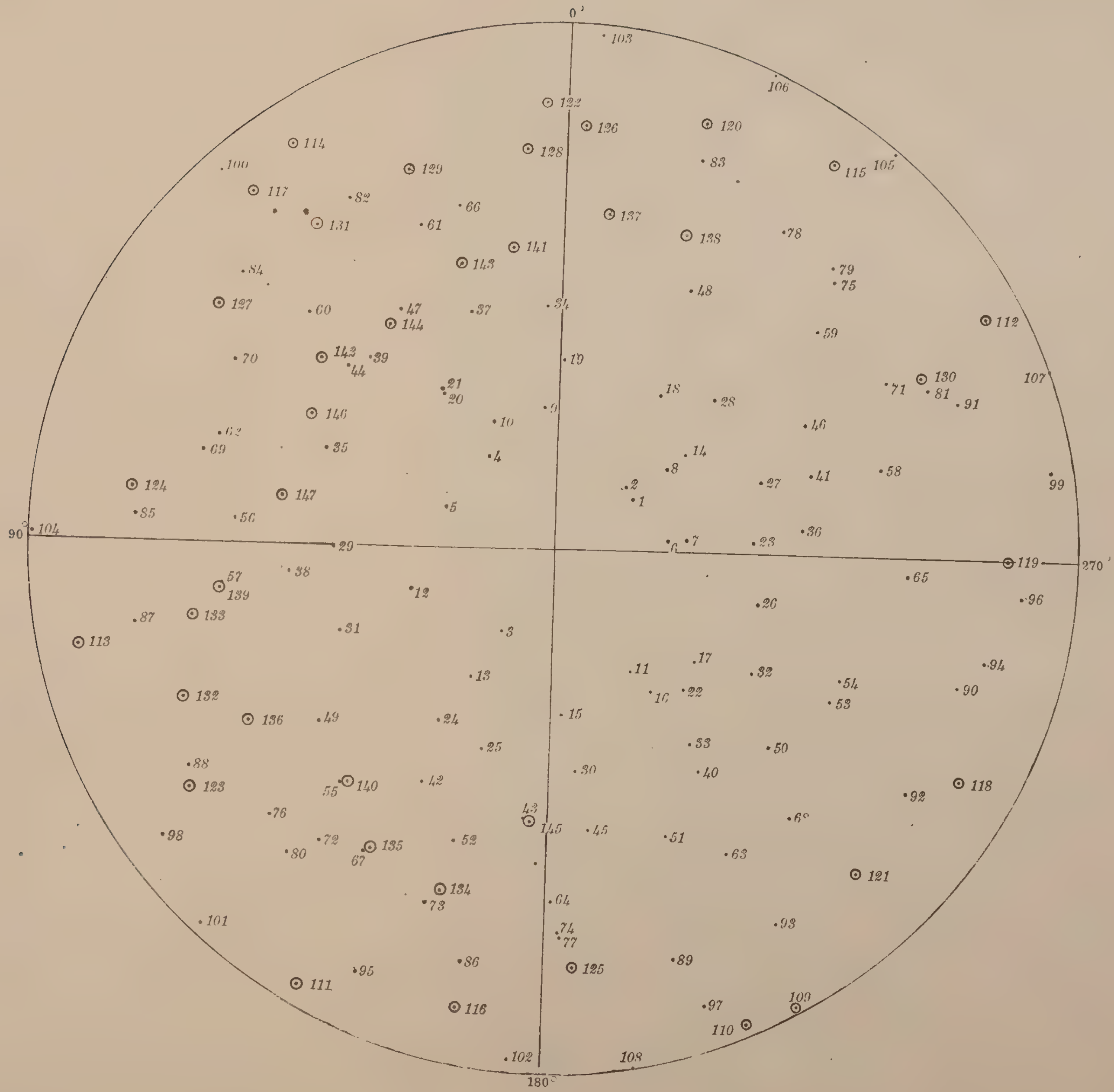
Polar Distance.	Hour-Angle.	Difference.	Zenith Distance.	Difference.	Refraction.
° ′	h. m. s.	m. s.	° ′ ″	′ ″	″
44 45	2 34 4·6		26 0 17		28·4
		2 29·1		30 16	
45 0	3 36 33·7		26 30 33		29·1
		3 13·8		39 45	
45 20	2 39 47·5		27 10 18		29·9
		3 8·4		39 6	
45 40	2 42 55·9		27 49 24		30·8
		3 3·4		38 30	
46 0	2 45 59·3		28 27 54		31·6
		2 58·7		37 55	
46 20	2 48 58·0		29 5 49		32·4
		2 54·2		37 24	
46 40	2 51 52·2		29 43 13		33·2
		2 50·0		36 53	
47 0	2 54 42·2		30 20 6		34·1
		2 46·0		36 25	
47 20	2 57 28·2		30 56 31		34·9
		2 42·2		35 59	
47 40	3 0 10·4		31 32 30		35·8
		2 38·8		35 34	
48 0	3 2 49·2		32 8 4		36·6
		2 35·3		35 10	
48 20	3 5 24·5		32 43 14		37·4
		2 32·0		34 48	
48 40	3 7 56·5		33 18 2		38·2
		2 29·0		34 26	
49 0	3 10 25·5		33 52 28		39·1
		2 26·1		34 7	
49 20	3 12 51·6		34 26 35		39·9
		2 23·2		33 47	
49 40	3 15 14·8		35 0 22		40·8
		2 20·6		33 29	
50 0	3 17 35·4		35 33 51		41·6
		3 26·1		49 41	
50 30	3 21 1·5		36 23 32		42·9
		3 20·6		49 5	
51 0	3 24 22·1		37 12 37		44·3
		3 15·6		48 31	
51 30	3 27 37·7		38 1 8		45·6
		3 10·7		47 58	
52 0	3 30 48·4		38 49 6		46·9
		3 6·2		47 29	
52 30	3 33 54·6		39 36 35		48·3
		3 1·9		47 1	
53 0	3 36 56·5		40 23 36		49·6
		2 57·8		46 35	
53 30	3 39 54·3		40 10 11		50·9
		2 54·0		46 9	
54 0	3 42 48·3		41 56 20		52·3
		2 50·4		45 46	
54 30	3 45 38·7		42 42 6		53·8
		2 47·8		45 24	
55 0	3 48 26·5		43 27 30		55·2
		2 42·6		45 2	
55 30	3 51 9·1		44 12 32		56·7
		2 40·4		44 43	
56 0	3 53 49·5		44 57 15		58·1
		2 37·5		44 23	

Polar Distance.	Hour-Angle.	Difference.	Zenith Distance.	Difference.	Refraction.
° ′	h. m. s.	m. s.	° ′ ″	′ ″	′ ″
56 30	3 56 27·0		45 41 38		59·7
		2 34·5		44 6	
57 0	3 59 1·5		46 25 44		1 1·3
		2 31·8		43 48	
57 30	4 1 33·3		47 9 32		1 2·8
		2 29·2		43 32	
58 0	4 4 2·5		47 53 4		1 4·4
		2 26·8		43 17	
58 30	4 6 29·3		48 36 21		1 6·0
		2 24·3		43 1	
59 0	4 8 53·6		49 19 22		1 7·8
		2 22·0		42 48	
59 30	4 11 15·6		50 2 10		1 9·5
		2 19·9		42 35	
60 0	4 13 35·5		50 44 45		1 11·3
		2 17·7		42 21	
60 30	4 15 53·2		51 27 6		1 13·1
		2 15·7		42 9	
61 0	4 18 8·9		52 9 15		1 14·9
		2 13·8		41 57	
61 30	4 20 22·7		52 51 12		1 16·8
		2 12·0		41 46	
62 0	4 22 34·7		53 32 58		1 18·8
		2 10·2		41 35	
62 30	4 24 44·9		54 14 33		1 20·8
		2 8·4		41 25	
63 0	4 26 53·3		54 55 58		1 22·7
		2 6·8		41 15	
63 30	4 29 0·1		55 37 13		1 24·9
		2 5·3		41 5	
64 0	4 31 5·4		56 18 18		1 27·1
		2 3·7		40 57	
64 30	4 33 9·1		56 59 15		1 29·4
		2 2·2		40 47	
65 0	4 35 11·3		57 40 2		1 31·7
		2 0·9		40 39	
65 30	4 37 12·2		58 20 41		1 34·2
		1 59·5		40 32	
66 0	4 39 11·7		59 1 13		1 36·6
		1 58·2		40 23	
66 30	4 41 9·9		59 41 36		1 39·3
		1 56·9		40 16	
67 0	4 43 6·8		60 21 52		1 42·0
		1 55·7		40 9	
67 30	4 45 2·5		61 2 1		1 44·7
		1 54·6		40 2	
68 0	4 46 57·1		61 42 3		1 47·7
		1 53·4		39 56	
68 30	4 48 50·5		62 21 59		1 50·7
		1 52·5		39 49	
69 0	4 50 43·0		63 1 48		1 53·9
		1 51·3		39 43	
69 30	4 52 34·3		63 41 31		1 57·2
		1 50·4		39 37	
70 0	4 54 24·7		64 21 8		2 0·6
		1 49·4		39 32	
70 30	4 56 14·1		65 0 40		2 4·3
		1 48·4		39 27	

Polar Distance.	Hour-Angle.	Difference.	Zenith Distance.	Difference.	Refraction.
° ′	h. m. s.	m. s.	° ′ ″	′ ″	′ ″
71 0	4 58 2·5		65 40 7		2 8·0
		1 47·6		39 21	
71 30	4 59 50·1		66 19 28		2 11·9
		1 46·8		39 16	
72 0	5 1 36·9		66 58 44		2 16·0
		1 45·9		39 12	
72 30	5 3 22·8		67 37 56		2 20·4
		1 45·1		39 7	
73 0	5 5 7·9		68 17 3		2 25·1
		1 44·4		39 3	
73 30	5 6 52·3		68 56 6		2 29·9
		1 43·7		38 58	
74 0	5 8 36·0		69 35 4		2 35·0
		1 43·0		38 54	
74 30	5 10 19·0		70 13 58		2 40·4
		1 42·3		38 51	
75 0	5 12 1·3		70 52 49		2 46·1
		1 41·7		38 47	
75 30	5 13 43·0		71 31 36		2 52·2
		1 41·0		38 44	
76 0	5 15 24·0		72 10 20		2 58·8
		1 40·5		38 40	
76 30	5 17 4·5		72 49 0		3 5·7
		1 39·9		38 37	
77 0	5 18 44·4		73 27 37		3 15·2
		1 39·4		38 34	
77 30	5 20 23·8		74 6 11		3 21·1
		1 38·8		38 30	
78 0	5 22 2·6		74 44 41		3 29·7
		1 38·4		38 29	
78 30	5 23 41·0		75 23 10		3 39·0
		1 38·0		38 28	
79 0	5 25 19·0		76 1 35		3 48·9
		1 37·5		38 23	
79 30	5 26 56·5		76 39 58		4 0
		1 37·0		38 21	
80 0	5 28 33·5		77 18 19		4 12
		1 36·7		38 19	
80 30	5 30 10·2		77 56 38		4 25
		1 36·3		38 16	
81 0	5 31 46·5		78 34 54		4 39
		1 36·0		38 14	
81 30	5 33 22·5		79 13 8		4 55
		1 35·6		38 13	
82 0	5 34 58·1		79 51 21		5 12
		1 35·4		38 10	
82 30	5 36 33·5		80 29 31		5 32
		1 35·0		38 10	
83 0	5 38 8·5		81 7 41		5 54
		1 34·8		38 7	
83 30	5 39 43·3		81 45 48		6 19
		1 34·5		38 6	
84 0	5 41 17·8		82 23 54		6 47
		1 34·3		38 5	
84 30	5 42 52·1		83 1 59		7 19
		1 34·1		38 4	
85 0	5 44 26·2		83 40 3		7 57
		1 33·9		38 3	

Polar Distance.	Hour-Angle.	Difference.	Zenith Distance.	Difference.	Refraction.
° ′	h. m. s.	m. s.	° ′ ″	′ ″	′ ″
85 30	5 46 0·1		84 18 6		8 38
		1 33·8		38 1	
86 0	5 47 33·9		84 56 7		9 31
		1 33·6		38 1	
86 30	5 49 7·5		85 34 8		10 30
		1 33·4		38 1	
87 0	5 50 40·9		86 12 9		11 41
		1 33·4		37 59	
87 30	5 52 14·3		86 50 8		13 9
		1 33·2		37 59	
88 0	5 53 47·5		87 28 7		14 59
		1 33·2		37 59	
88 30	5 55 20·7		88 6 6		17 18
		1 33·2		37 58	
89 0	5 56 53·9		88 44 4		20 16
		1 33·1		37 58	
89 30	5 58 27·0		89 22 2		24 10
		1 33·0		37 58	
90 0	6 0 0·0		90 0 0		29 11

The hour angles and zenith distances in the Table are those which would geometrically correspond to the given polar distances; the refractions, calculated from Bessel's mean refractions, are to be deducted from the tabular zenith distances if the apparent zenith distances be required.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PROCEEDINGS

OF THE

Cambridge Philosophical Society.

ANNUAL GENERAL MEETING.

Oct. 30, 1882.

DR CAMPION, VICE-PRESIDENT, IN THE CHAIR.

The following were elected officers and new members of Council for the ensuing year:

President.

Mr J. W. L. Glaisher.

Vice-Presidents.

Professor Babington.
Professor Newton.
Professor Cayley.

Treasurer.

Dr J. B. Pearson.

Secretaries.

Mr J. W. Clark.
Mr Coutts Trotter.
Mr W. M. Hicks.

New Members of Council.

Dr W. M. Campion.
Mr E. Hill.
Mr J. N. Langley.

Mr J. R. Green, Trinity College, was balloted for and dul elected an associate of the Society.

Mr Glaisher having taken the Chair, the following communica tions were made to the Society:

(1) *On the construction of a negative eye-piece.* By Dr J. B Pearson.

I wish to be allowed to offer a few remarks on the constructio of the negative astronomical eye-piece: only as designed for th use of abnormally short-sighted or long-sighted persons. For goo average sight, I have no doubt that the existing construction is al that is to be desired.

Sir E. Beckett (*Astronomy without mathematics*, p. 301) says "All eye-pieces are made adjustable for different eyes by the eye glass sliding nearer to the field-glass for short-sighted eye which require the rays of each pencil to diverge a little instead o being parallel." It is true this is the case with the sextant-circl telescopes which I use, the two German ones having positive eye pieces, and the French a negative one: but it is not so with th common astronomical negative eye-pieces, such as are sold i England. Now with powers up to say 70, and an aperture o about 3 inches, I have found I got very good results by focussin in the usual way; but with a power of 120, I found I could get n satisfactory definition; and as a fault, obviously the same, cam out with different object-glasses, I was led to examine the con struction of the eye-piece to see whether part at any rate of th fault might be there.

Theoretically, and in practice, for perfect eyes, the focus an diaphragm or stop of a negative eye-piece are placed between th two lenses: but if a short-sighted person adapts the focus of th telescope to his own eye, he moves the whole eye-piece by mean of the large screw. This really means that he pushes forward th field-glass, *i.e.* the lens nearest the object-glass, so that it stan nearer to the object-glass: by this means the field-glass intercep portions of the pencils coming through the object-glass, which o an average are more nearly parallel to their axis: the focus of th rays passing through the object-glass and field-glass is thus throw farther from the field-glass than it was before; and, as the distan between the field- and eye-glass is unchanged, also nearer to th eye-glass: they thus diverge after passing through the eye-glas and are consequently fit for vision by a short-sighted eye.

This process answers very well in ordinary cases and wit moderate magnifying powers, but if extreme precision is neede it seems to me to be defective. The field-glass foreshortens slight the focal length of the object-glass, but its special purpose is

chromatise the rays, if I may use such a phrase, in order that they may be achromatic when they have passed through the eye-glass. The place of its focus however, as far as I can see, ought not to be changed: it is one easily ascertained: it is the focus for rays not parallel, but with the convergence given them by the object-glass at the point where the lens intercepts them to the greatest advantage. Knowing the focal length of the object-glass and of the field-glass, it seems to me that this point ought to be carefully ascertained by the maker, and indicated by the stop: my French sextant-telescope has wires at this point which I can get perfectly distinct by adjusting the eye-glass; and I think without distortion, though theoretically it seems this ought to be a result. Any how the eye-glass should be made moveable as I thought myself, or else should be altered in form as was suggested to me by a friend, i.e. made less convex for a short-sighted person, more so for one with long-sight; so that the person using the telescope, by first focussing on the stop, will have as perfect vision as possible. When I mentioned the difficulty, to a friend or to my maker, though I do not remember which, the remedy he suggested was, that I should focus on the stop: and this is no doubt correct, but I am not sure that this prescription is sufficient of itself. Mr Coddington says that the stop is to be placed half-way between the two lenses, the field-glass and the eye-glass: but this is not actually done in practice; it being placed much nearer to the eye-glass than to the other: and also that the two lenses should be *meniscus*-shaped, the field-glass with the surfaces of radii 4 and 11, the convex side turned towards the object-glass: and the eye-glass with surfaces 6 and 1, making what is called a *crossed* lens, the convex side here also being turned towards the other glass: and both these principles, I may add, seem borrowed from the rules given in Sir Geo. Airy's paper on "Achromatic Eye-pieces" in Vol. III. of our *Transactions*, read in 1827. To my mind, the stop ought to be fixed by the relative focal-lengths of the object-glass and of the field-glass: and the eye-glass may vary in distance from it according to the nature of the observer's eye.

The following objection has been taken to making the eye-glass moveable: that the achromatism is injured by doing so: but it seems to me that the great thing is definition, and that the eye itself may possibly rectify a slight defect in this respect. At the same time an alteration of the form of the eye-glass to suit a short-sighted eye seems a most proper thing to introduce: the focussing on the stop being reserved to determine finally its position. A positive eye-piece is, I believe, not achromatic.

To sum up: my principle is this. The field-glass, of a defined focal length, placed in front of an object-glass of defined focal length, produces the best defined image at a definite point. The

stop should be placed here. The eye-glass should be adapted, i form and position, to receive this image to the best advantage Not till then should the large focussing screw be used to bring th stop, and practically the eye-piece, into its proper place.

I believe I am justified in thinking that achromatism may be comparatively speaking, neglected: first, because it is allowed tha positive eye-pieces are not achromatic: secondly, because it seem probable that chromatic aberration shews itself principally whe the lenses are large, which those of the eye-piece are not. I ma add that this kind of eye-piece seems to have been designed by it inventor, Huyghens, to correct distortion, and not chromatic aber ration.

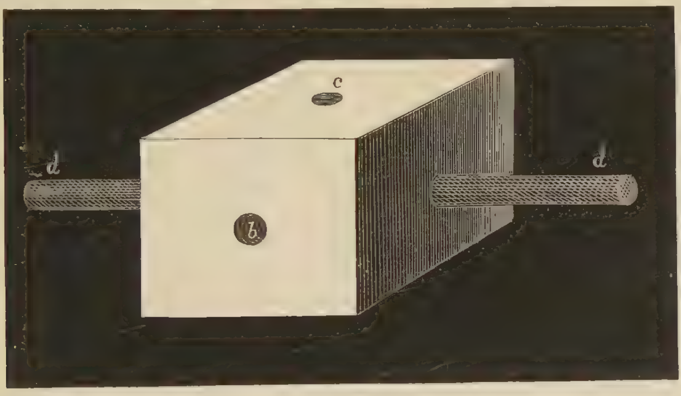
(2) *On the circumstances producing the reversal of spectra lines of metals.* By Professors LIVEING and DEWAR.

Our object in investigating the reversals of the lines of terres trial elements has been to trace the parallel between the condition of the elements as they exist in the sun, and those in which w can place them on earth; with a view to illustrate more fully th problems of solar chemistry. A knowledge of the reversible line may also help us to distinguish those rays which are directly du to the vibrations communicated to the luminiferous ether by th freely moving molecules themselves from those which are produce by superposition of waves, or by some peculiarity in the mode i which the molecules are put in motion, or by some strain upo them, such as the violent Leyden jar discharge might give, whic would not be an easily reversible action.

In a former communication we brought before the Society th results of our observation of the reversal of the lines of some of th more volatile metals when the continuous spectrum of the ho walls or end of an iron or porcelain tube was observed throug vapour filling the tube. As the temperature of the tubes was onl that of a crucible furnace (about 1500° C. at the outside), it wa a very limited number of the lines even of the volatile metal which could be reversed in that way, because the relative strengt of the emissive (and therefore of the absorptive) power of a vapou for particular rays often varies considerably when the temperatur is changed. Thus the vapour of sodium in an ordinary flam emits little radiation except that of the yellow pair of lines an only absorbs the same; and it is only at higher temperature such as that of the electric arc, or that of a flame locally in creased by the introduction of an endothermic salt such a a chlorate, that the other pairs (or groups) of lines red, orang green and blue are seen either bright or reversed. We wan then a source of light which shall give a more or less con tinuous spectrum of an intensity greater than that of the lines t

be observed, and the metallic vapour not only in a layer of some thickness, but heated up till it is capable of freely emitting the light of those lines which we want to reverse. In the case of the less volatile metals such as iron and aluminium it would not be expected that these conditions should very easily be fulfilled.

After trying the oxy-hydrogen jet to ignite the bottom of a tube bored in a block of lime, which gave us reversals of the well-known green and orange bands of lime, and of the blue line of calcium which was barely reversible in tubes heated in a crucible furnace, we turned our attention to the electric arc as a source of heat both to give the requisite bright background and to vapourize the metals examined, but enclosed it in a block of lime or magnesia by which we were able to maintain a considerable amount of



a. block of lime or magnesia.
b. hole bored to the centre of block through which observations were made of the arc.
c. hole by which metals were dropped into the arc, usually covered with a piece of lime.
d.d. carbon rods forming electrodes of dynamo-electric machine.

refractory metal in a state of vapour, and from the tube-like form of the opening through which the arc was viewed could observe the absorption of a tolerable thickness of such vapour gradually diminishing in temperature as it receded from the arc. The greater part of our observations were made with some one or other of several modifications of this apparatus.

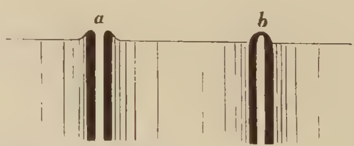
For the ultra violet part of the spectrum photography was used. Generally 15 photographs were taken on the same plate in order to vary the time of exposure, as good impressions of faint lines cannot be obtained without over exposing the strong lines: and also in order to catch different phases of effect as the metal introduced evaporates.

The reversals thus observed may be classed under several heads.

I. Reversals by the expansion of the line observed or as they may be called self-reversals. These are the reversals most generally known. The sodium lines are frequently so reversed, indeed always when a sufficiently volatile salt of sodium such as the carbonate or chloride is held in a gas flame. If sufficient sodium is in the flame the lines are widened, and the less dense and less hot sodium vapour outside the flame produces a narrow absorption line down the middle of the bright yellow band. In this way the metal itself gives the background against which the reversed line is seen; and in order to produce the result the line must be more expanded at the higher temperature than at the lower. That does not always happen, but it is so common that many reversals may be seen in this way. The magnesium line of wave length 2852 has great power of expansion and is always seen reversed in this way when the arc is taken in a magnesia crucible as in Plate I. fig. 1.

One set of the photographs exhibited are original negatives of the arc spectrum shewing reversals of this class in the case of lines of magnesium, thallium, indium, tin, aluminium, antimony, lead and zinc. They are only a selection out of a very large number taken by us but sufficient to shew the characters of this class of reversals.

Professor Hartley has lately (*Proc. Roy. Soc.* XXXIV. 84) called attention to the pseudo-reversals of this class which may be produced in the case of a strong line by over-exposure. It is well known that over exposure (solarization as we used to call it formerly) produces such an alteration in the sensitive preparation of the photographic plate that the over-exposed parts cease to be de-



velopable, so that a very strong line may appear white in the negative where it ought to be black, but with a dark border and so give the appearance of a reversed line. Professor Hartley finds it difficult to distinguish real reversals of the class we are now discussing from these pseudo-reversals. His difficulty has not occurred to us, first because we have always been in the habit of taking our photographs in series with varying exposure, in order to get impressions both of the feeble lines in some and of strong lines in others; and secondly because we almost always close part of the slit of the spectroscope with a shutter so that the

image is cut off sharply by the shadow of the shutter. Strong lines extend into the shadow more or less, and if there is a real reversal the extension of the reversed part into the shadow is trumpet-shaped (*a*), whereas if it is only a pseudo-reversal it is closed (*b*).

We would call especial attention to the enormous expansibility of the magnesium line w. l. 2852. The limits are quite indefinable either up or down the spectrum and the absorption (*i.e.* the reversed line) is a broad band, sometimes reaching up to the solar line U as seen in Plate I. fig. 2, which is a negative photograph of the same part of the arc as fig. 1, but had a fragment of magnesium dropped into it at the moment it was taken. You might fancy that magnesium were the substance which produces the absorption of the sun's rays above U, if it were not that the light reappears in the case of magnesium above the dark band as well as below it while the sun's light seems to be quite cut off above U. The cessation of the solar spectrum above the magnesium line may be due to the general absorption of the lines of iron and other metals which are far more numerous in that region than in the visible part of the spectrum, and it is just possible that a very long exposure of sensitive plates may yet reveal some extension of the solar spectrum beyond U.

In these cases of self-reversals the light of the line is only partially absorbed but there are other cases of

II. Complete reversal, where the lines are seen only as dark lines. To this class belong the reversals before described when the lines of volatile metals were seen dark against the bright background of the hot bottom and sides of the tubes heated in a furnace. But the arc itself does not generally give such a background. It gives generally a discontinuous spectrum consisting of bright lines with only a very faint continuous spectrum. There are however some parts of the arc, as we used it, which are so full of fine closely set lines that it serves as a good background against which a dark reversed line can be seen. The tail of fine lines formed by the ultra-violet Cyanogen bands is such a region. Another is in the neighbourhood of Q where there are a great many fine iron, manganese and chromium lines, and where also the fine lines of the water spectrum add to the effect so as to give something like a continuous spectrum.

One of our photographs shews the reversal of a Thallium line near L. In the negative photograph it is a simple absorption line. Another photograph shews silver lines near Q reversed as mere white lines on the general spectrum. In some cases we have succeeded in getting a good background by bringing up the hot carbon pole into the line of view. In that case the larger part of the bright metallic lines disappear and the con-

tinuous spectrum becomes much stronger, so that the reversed lines are more readily seen. We have various plates shewing complete reversals of the lines of several metals in one or other of these ways. The ultra violet lines of potassium form a series extending at decreasing intervals to about the solar line U. They are all reversed, appearing only as white lines in the (negative) plates. The least refrangible is a double line just below O. Probably they are all double in reality, and harmonically related to the violet pair. Plate II. shews most of these potassium lines reversed.

In other plates are seen complete reversals of copper and silver, and lead lines. Some of our plates shew complete reversals of a great many iron lines. In two of these plates the reversals were obtained by putting an iron wire into the arc through one of the carbons, which was perforated for the purpose. Some of the lines are greatly expanded, but they appear only as absorption lines; they have no bright wings. In another case the iron was introduced into the arc as ferrocyanide of potassium, and the lines are sharp clean fine lines just like the Fraunhofer lines in the solar spectrum, the continuous background being heightened by the potassium, which seems to give some continuous spectrum in the ultra-violet region. Plate III. shews a great many iron lines so reversed.

III. Another class of reversals are produced by the vapour of one metal seen against a bright background given by the expansion of a bright line of some other metal. For this purpose the expanded lines of magnesium are most effective from their great breadth. We have several plates shewing sharp clean reversals of iron lines against the continuous light of the expanded magnesium line, w. l. 2852; one shews lines reversed in this way against the bright expansions on both sides of the absorption band of magnesium. One of the plates shews some iron lines near M and chromium lines reversed in this way. Plate I. fig. 2 shews the principal iron lines between S and U reversed against the expanded magnesium line w. l. 2852.

IV. A fourth mode of obtaining reversals we cannot so easily explain. It is by allowing a very gentle stream of hydrogen, coal gas, or ammonia, to pass into the arc either through one of the carbons perforated or through a separate opening into the crucible. The effect of such a current is generally to sweep away most of the metallic lines, make those which remain fainter, and increase, relatively at least, the continuous spectrum, and reverse many lines. The character of the effect is fairly seen in a photograph, Plate IV., of the part of the spectrum near U, in which the magnesium line (2852) appears about as strong as usual in a magnesia crucible, but wholly as an absorption, while the other magnesium lines are still bright. Many of the iron lines are reversed in this photo-

graph, and so are some less refrangible calcium lines. No doubt many of the other lines which have disappeared are not seen because absorption just balances emission. Indeed by varying the current different effects may sometimes be produced, the same line appearing at one time bright, at another time dark, and at another time not at all. We have seen the calcium lines at H and K, and the strong blue triplet just below the well-known (indigo) calcium line at wave length 4226 all bright, while the indigo line itself was quite invisible, it was neither bright nor reversed. This and similar effects are no doubt due to differences in emissive power at the different temperatures of the emitting and absorbing vapour. At the higher temperature the emissive power of calcium for H and K and the blue triplet is relatively greater than for the indigo line, while at the lower temperature of the absorbent vapour the reverse holds; so that the emission for the indigo light is balanced by absorption, while that of the other rays is not balanced. In like manner we have frequently seen H when K was quite invisible, sometimes K reversed when H was not reversed.

It is not at first sight easy to explain the action of the gas. Probably hydrogen is the chief agent in the case, for a mere current of air has no such effect. The gas may act by helping to diffuse the metallic vapours, diminishing their density in the arc, and increasing it in the tubular part of the crucible without too much lowering the temperature. Hydrogen gas also maintains a reducing atmosphere, preventing the oxidation of the metallic vapour. It also forms compounds with some metals, notably with magnesium and the alkali metals, as well as with the carbon of the electrodes, which are probably all endothermic, and whether that be so or no, must affect the distribution of the temperature in the arc and tube. In the case of expanded bright lines, when they are not wholly swept out by the gas the effect is to diminish the wings, and as absorption does not begin at the wings, this can only be by diminishing the range of emission, probably by diluting the metallic vapour. The amount of heat absorbed by such a small mass of gas will not lower the temperature much. Moreover a current of chlorine has usually the opposite effect to that of a current of hydrogen, increasing the strength of the bright lines, probably by assisting the volatility of the metals, and so increasing the quantity in the arc. That the explanation here offered of the action of hydrogen is correct, is borne out by the behaviour of mixtures of metals. Thus an alloy of zinc with a little lead gives far sharper and cleaner reversals of the lead lines than lead alone does. When lead alone is put into the arc in the crucible the lead lines come out very strong and diffuse, and the emission is not nearly balanced by the absorption in the (comparatively) cool tube; but when the alloy with zinc is used the lead lines are

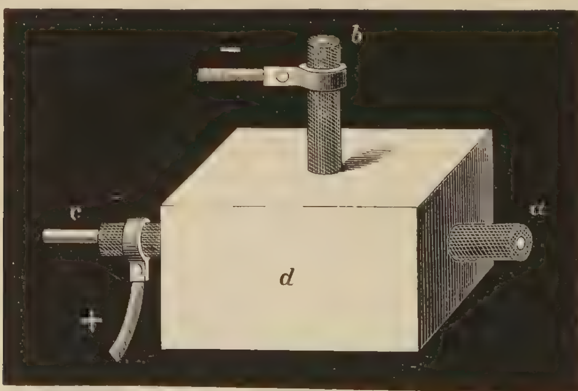
much less expanded and the emission much more nearly balanced by the absorption.

By inclosing the arc in a block of lime or magnesia we have found its steadiness very greatly increased, and the mass of metallic vapour which can be maintained at a temperature approaching to that of the arc much enlarged, but it cannot be said that that temperature is at all under control, and the walls of the crucible are almost always cooler than the contents.

V. We have lately used a carbon tube, passed quite through a block of lime, and made it the positive electrode of the current, while the other electrode is a carbon rod passed into the lime at right angles to the tube so as to meet it in the middle.

In this way the outside of the tube becomes intensely heated. the heat is retained by the jacket of lime, and the interior of the tube gradually rises in temperature, and attains in the central part a very high point. By stopping the arc it can be made to pass through the same stages of temperature in the inverse order. Observations are made by looking down the perforation. When the light issuing from the tube is projected by a lens on to the slit of a spectroscope, the heated walls of the tube give at top and bottom a continuous spectrum, against which various metallic lines are seen reversed, while in the central part, when the tube is open at the farther end, the spectrum is discontinuous, and the metallic lines seen reversed against the walls at top and bottom appear as bright lines. Plate V. is a negative photograph shewing this effect.

The apparatus employed is thus constructed: A rod of carbon, *a* in the figure, 15 millims. in diameter, perforated down its axis



with a cylindrical hole 4 millims. in diameter, is passed through a hole in a lime block *d*, and is connected by means of a copper clip with the positive electrode of a Siemens dynamo-electric machine;

another carbon rod *b*, unperforated, is passed into the lime block through a second hole at right angles to the first, so that the end of the rod *b* meets the rod *a* in the middle of the block of lime. The rod *b* is connected with the negative electrode of the dynamo machine, and after contact is made between the two carbons is raised a little so that the arc discharge continues between the two carbon rods within the block of lime.

By passing a small rod of carbon into the perforation from the farther end, a luminous background can be obtained all across the field, and then, as the walls of the tube are hotter than the metallic vapours between them and the eye, the metallic lines are only seen reversed. A very slight alteration in the position of the small carbon rod forming a core to the tube makes the lines disappear, or reappear, or shew reversal, and as the core is adjusted by eye observation before photographs are taken, all the conditions of the experiments are known and are under easy control.

By this arrangement we are able to make observations as the temperature rises and as it falls, and so to trace the influence of temperature in many cases in which the extent of that influence was before doubtful. The temperature attainable is doubtless far below that of the arc, but still it is quite sufficient to maintain iron and aluminium in the state of vapour, and shew the reversal of the lines of these elements with singular sharpness. The temperature of the interior is sufficiently high to transform the diamond into coke, even in a current of hydrogen, and the result may be taken as proving that the temperature is above that of the oxyhydrogen flame. We have taken photographs of the violet and lower part of the ultra-violet spectrum given by the tube at successive intervals while the temperature was rising, and noted the following results. When commercial carbons were used, which always contain iron, aluminium, manganese, calcium and magnesium as well as traces of other metals, the first lines to be seen as the temperature rose were the potassium lines, wave-length 4044–6, next the two aluminium lines between H and K became conspicuous, then the manganese triplet about wave-length 4034, and the calcium line, wave-length 4226, then the calcium lines near M and an iron line, probably M, between them, and then gradually a multitude of lines which seem to be all the conspicuous iron lines between O and *h*. At this stage, when the core is inserted to give a background, the bright continuous spectrum is crossed by a multitude of sharp dark lines, vividly recalling the general appearance of the solar spectrum. In the higher region the continuous spectrum extends beyond the solar spectrum, and the magnesium line, wave-length 2852, is a diffuse dark band, while all the strong iron lines about T, and the aluminium pair near S, are seen as reversed lines. The behaviour of the calcium

lines H and K is peculiar. These lines are often absent altogether, when the line wave-length 4226 and the two near M are well seen, and when the two aluminium lines between them and many of the iron lines are sharply reversed. Even the introduction of a small quantity of metallic calcium or calcium chloride into the tube did not bring them out reversed. They were only seen as bright lines, not very strong, when the small rod was removed. The lithium lines at 4603 and 4131 are often bright when many other lines in the neighbourhood are reversed, and must therefore be regarded as relatively difficult of reversal. As a rule the lines less refrangible than 4226 are balanced as to their emissive and absorptive power and, therefore, disappear, while the more refrangible are reversed.

Both the indium lines 4101 and 4509 are persistently reversed, and so are several lead lines. Tin gives lines, of which some are reversed, in highly refrangible regions besides a channelled spectrum, and silver gives a fine fluted-looking spectrum in the blue. Chloride of calcium gives a striking set of six or seven bands between L and M, which may be seen both bright and reversed.

VI. Occasionally a double self-reversal of lines occurs, *i.e.* in the middle of the reversed or absorption line a bright line appears. We have many times seen this but not often photographed it. We have a plate which shews it in the case of the magnesium triplet between K and L. The photograph is that of the arc itself projected on to the slit of the spectroscope by a lens, and was taken at the moment when a piece of magnesium was dropped into the crucible. This has caused such an expansion of the bright lines that they have extended beyond the nearest two cyanogen bands and caused them to be reversed. On this bright background the magnesium lines themselves are reversed as a dark band in the centre of the field, and on this dark band are three narrow bright lines forming the second reversal. It is not difficult to understand how this occurs. The magnesium is very quickly evaporated and part of the vapour is driven by the expansion up the tube of the crucible before it has got very hot, and it drives before it some hotter vapour previously in the hottest part of the crucible. The cool vapour stops the radiation of the arc and the hotter vapour in front of it of course appears bright. The effect is little more than momentary. In this case the cool vapour was only just around the arc, for at top and bottom the same narrow lines which are bright in the middle of the field (black in the negative photograph) are reversed in the top and bottom.

It is worthy of remark that the facility of reversal of lines appears, speaking generally, to increase as the wave-length diminishes. This is very notable in the case of iron. It seems

as if the fundamental vibrations, *i.e.* those most easily assumed by the molecule of the metal in a free state were in the higher region of the spectrum. As if in fact they might be vibrations of which we have as yet perceived only lower harmonics. The great line of magnesium at wave-length 2852 certainly seems a fundamental one, if we may judge by its persistence, strength, and facility of reversal.

It is, however, by no means always the strongest lines which are most easily reversed, but rather those which are both persistent and strong. The ultra-violet lines of iron, which are so expanded and reversed in our photographs, are present, but not at all specially conspicuous, amongst the lines in the spectrum of the spark, but they are strong lines always in the arc. It seems as if in the spark, especially when a Leyden jar is used, violence were done to the metals, different in some way from that due to mere elevation of temperature, which gave them violent vibrations but not exactly those which they most readily take up when under less constraint. It is remarkable how easily the well-known pair of aluminium lines between H and K are reversed in the arc as well as the other pairs near S and above, and yet the metal in the arc does not shew at all either the triplet near N (w. l. about 3585, 3598, 3605) or the lower lines which are strong in the spark spectrum. The quantity of metallic vapour is of course much less in the spark than in the arc, but it is much more violently, probably much more irregularly, agitated.

A very important factor in chemical action is the pressure to which the materials are subject, and we have not overlooked its importance in regard to solar chemistry. The difficulties of operating at pressures differing much from that of the atmosphere have hitherto delayed the extension of our observations in that direction, but we hope that those difficulties will not prove insuperable and that we shall be able on a future occasion to lay before the Society some observations on the effects of pressure on the phenomena of reversals.

Nov. 13, 1882.

MR GLAISHER, PRESIDENT, IN THE CHAIR.

The following communications were made to the Society:

(1) *On the Structure of the Spleen.* By J. N. LANGLEY, M.A., and C. S. EVANS, B.A.

(2) *On the continuity of the protoplasm in the motile organs of leaves.* By W. GARDINER, B.A.

Any one who has studied the development of biological research for the last few years cannot but be struck by the gradual evolution of grand generalisation: the similar explanation of dissimilar phenomena: the classification of hitherto unclassifiable facts. And to start on what we should have said was the widest possible basis, nothing can illustrate this statement better than the gradual and complete break down of that hard-and-fast line of distinction which existed between the animal and vegetable kingdoms. Step by step we have seen each difference disappear, and to those of us who have followed it, the reason has been plain. We study the properties of protoplasm. We shall expect an exhibition of those properties. It is a matter of secondary importance where that protoplasm happens to be. True it is that in different organisms some property or properties will be accentuated more than the rest: the maximum and minimum of each will constantly alter as we pass in review over the whole; but among the many variations we shall recognize the one true air: the difference will be one of degree, not of kind. And so when the student of plant life follows the properties of protoplasm, as set forth in a well known text-book, it becomes at once apparent that they are all exemplified in his kingdom: a *Hœmatococcus* is to him what an *Amœba* is to the Comparative Anatomist.

We can, then, no longer attempt to establish any binding distinction. Any general statements made are open to exception. Thus, as Bergh has very recently shewn, the possession of a cell wall and the power of manufacturing complex proteid material from simple chemical compounds must no longer be regarded as characteristic peculiarities, for they are both shared by the *Cilio-flagellata,* and though perhaps Darwin makes rather an astonishing statement when he compares a young growing radicle to a burrowing animal, and forcibly suggests that its tip acts like a lowly

developed brain, yet it is quite apparent that in plant life we do have here and there glimpses of that quick response to stimuli, that wonderful irritability and spontaneity which so emphatically distinguishes animal life. I should like to bring forward one or two examples. Who in watching the movements of a swarmspore does not notice how quickly it progresses, and yet how it avoids every obstacle? Fertilisation again shews us the same thing. Why is it that the antherozoid will always go straight to the oosphere, or that two conjugating cells of *Spirogyra*—two different individuals as they are—will each protrude out their cell walls, contract their protoplasm, pass over and fuse? Is it as in the yeast that the protoplasm—vibrating with that activity which we call life, communicates its vibrations to the medium and allows its fellow protoplasm to respond; or in the case of the swarmspore does it avoid those objects which do not answer to the same note as its own, or seize on the food particle with whose cry it has been acquainted from its youth up? We cannot tell, for here our knowledge fails.

Sometime ago I noticed an interesting phenomenon in the swarmspores of *Saprolegnia*. As is well known, these asexual cells are produced by the segmentation of the protoplasm of one of the swollen hyphal branches. When matured the wall of the sporangium gives way, the swarmspores escape, and after swimming about actively for some minutes become quiescent and develope a cell wall. But it may happen that the aperture in the sporangium wall becomes shut again before all the swarmspores have passed out. In that case, whereas the freed spores will come to rest very quickly, those still in the sporangium will continue swimming round and round their prison for some comparatively long time, and will at length in their turn come to rest.

Of the many phenomena however which connect animal and plant life none are so striking to every mind as those connected with movement, and it is of movement that I have to speak—the movement of leaves. It was Sachs and Pfeffer in Germany and Charles Darwin in England who more especially drew attention to the spontaneous, periodic and irritable movements of leaves. Thus the so-called sleep movements are exhibited by a very great number of plants. In the daytime the leaves will be expanded. At night they will close in various ways. Automatic and irritable movements are somewhat less widely distributed—the telegraph plant is a good example of the first, the sensitive plant of the second. The mechanism of all these movements is essentially the same, viz. that at the base of each leafstalk or leaflet there is a flexible joint usually pronounced and well defined from the rest of the tissue.

I should like to draw attention to some of the physiological and morphological phenomena as displayed by one of the best

examples, viz. by the sensitive plant, *Mimosa pudica*. The compound leaf of this plant is of that form known as bipinnate; two or four secondary petioles spring from the main petiole of the leaf; each such secondary petiole bearing from fifteen to twenty-five pairs of leaflets, all the parts being connected the one to the other by swollen joints or pulvini. In the daytime the leaf assumes an expanded position, the main petiole forms an acute angle with the upper half of the stem and the leaflets are open, while at night the leaf falls and the leaflets close. It has assumed its nocturnal or sleep position. If in the daytime the leaf be touched or otherwise sufficiently stimulated, it will fall and assume a position externally identical with that of sleep. After some minutes it will rise and become expanded only to fall again upon the application of a fresh stimulus, but if the stimulus be too frequently repeated, it will soon become insensible to the action and will no longer respond. If placed for some time in the dark, it will execute spontaneous movements, the leaf alternately rising and falling. Herein it resembles the telegraph plant, except that the latter performs its spontaneous movements irrespective of the presence or absence of light. In Mimosa the spontaneous periodicity is during the daytime almost entirely concealed by the contractile part being very sensitive to the action of light. If it be not supplied with oxygen it will lose its irritability and become asphyxiated, or if exposed to the action of chloroform or ether, it will soon be rendered insensible and will no longer react. This is the physiological side of the question.

Turning to the microscopical structure of such a pulvinus—one notices that although three vascular bundles are apparent on both the stem side and petiole side of the pulvinus—in that organ itself, the three unite and furthermore become thinner, so as to form a single axial bundle, which, as Sachs points out, lies in the median plane of the organ and forms the neutral axis of curvature. The whole bundle is flexible, slightly extensible, and but little lignified. It is surrounded by a succulent mass of parenchyma, consisting of roundish cells of about eight layers thick covered by a badly differentiated uncuticularised epidermis from which stomata are absent. The parenchyma cells immediately under the epidermis are packed close together with no intercellular spaces, but as one approaches the bundle, spaces of larger and larger size become apparent, all of which are in communication one with the other. The cells of the under or more bending side are thin walled—the upper are thick. All the cells are very conspicuously pitted, especially on their longitudinal walls, and they each contain at least one nucleus and a large drop or drops of tannin. This is the morphological side.

Now as to the cause of these movements. If a spirogyra filament be examined it will be seen that the transverse wall between any two cells will be perfectly horizontal and at right angles to the long axis of the whole filament. If, however, one of the cells happens to be unhealthy from the attacks of parasites or otherwise, then the walls of the healthy cells on either side will no longer be horizontal but will project convexly inwards into the injured cell. The cubical contents of the healthy cells will in fact become greater—that of the unhealthy less. Now this is due to the fact that the layer of protoplasm surrounding the cell wall—the primordial utricle as it is called—possesses the power of absorbing into its interior large quantities of water, and thus becoming turgescent. In healthy cells the hydrostatic pressure of the one is resisted by the equal pressure of its neighbour. The wall between them will remain horizontal. But if the protoplasm be unable to maintain its turgidity, then its resistance to pressure will be less, and the common wall will be forced inwards by the increased pressure caused by the additional quantity of water which the neighbouring cell immediately takes up. And this hydrostatic pressure not only keeps the primordial utricle close to the cell wall, but actually expands the elastic cell membrane, as can easily be proved by treating such a cell with any substance which will cause a loss of turgidity, *e.g.* a solution of nitre. Thus any stimulus which will cause an increase or decrease of turgidity will also cause a contraction or expansion of the elastic cell wall. And this is what happens in Mimosa. We may regard the expanded position as the resultant of two equal and, for the time, opposite forces; the weight of the leaf and the pressure exerted by the cells of the upper half of the pulvinus tend to force the leaf down, while the turgescence of the cells of the lower half exactly counteract this downward pressure; but by an appropriate stimulus, once cause the lower cells to lose their turgidity, and the equilibrium is destroyed: the downward pressure now exerts its force: the leaf falls. These statements are supported by facts. A stimulus causes a loss of turgescence and an escape of water.

Now as to the propagation of the stimulus. When the main pulvinus or the terminal leaflets or any part of the leaf is stimulated there is a contraction of the whole leaf. But to limit our inquiries we may ask, How is the stimulus propagated from cell to cell in any one pulvinus. The usually received explanation is that as a result of a stimulus to any given point of the pulvinus the cell or cells affected suffer a disturbance of their cell equilibrium, there is a diminution of turgidity and water escapes. This water as it passes from cell to cell in turn upsets the equilibrium of each, causing a disturbance which is gradually propagated to the other

parts of the organ. The water was the cause of the propagation of the stimulus. This explanation is, however, unsatisfactory. That a stimulus is attended by an escape of water into the intercellular spaces is certain, for on stimulation the under side of the pulvinus is seen to assume a darker green colour due to the fact that the escaping water displaces the air in the intercellular spaces. But Pfeffer, who described the phenomenon, says himself, that when he touched a point of the irritable side he saw the darker colour spread instantaneously from the point of contact. We cannot imagine that the mere passage of water from cell to cell can explain this. We should have to assume such a great velocity and such a high pressure that rupture of the tissue must occur. Some other means of propagation of the stimulus must take place. The water must be the effect, not the cause. From a consideration of this and similar phenomena, Professor Sachs suggested to me that I should investigate in as complete a manner as possible the microscopical structure of pulvini, and endeavour to ascertain among other things how this propagation of the stimulus occurred. My results in the main confirmed those of other observers. One important additional fact which struck me was the uniform occurrence of very numerous pits on the walls of the parenchymatous cells, and it seemed not improbable that, through these pits and by means of fine protoplasmic filaments, continuity of the protoplasm from cell to cell was established. That this is the case I have succeeded in demonstrating.

My method of investigation depends on the fact that cellulose but not protoplasm is dissolved by strong sulphuric acid. This fact had been made use of by Sachs to demonstrate the continuity of protoplasm in sieve tubes. Mine is a modification of his method. The usual plan was to treat thin sections with Iodine by which means the protoplasm was stained. They were then mounted in strong Sulphuric acid—the cell wall dissolves, the stained protoplasm is left. The method is however open to two objections—First, the protoplasm is not sufficiently deeply stained; Secondly, one cannot regulate the action of the acid which in a short time will begin to act on the protoplasm also. I divided the process into two parts. The sections were first cautiously treated with Sulphuric acid and well washed with water to get rid of the reagent. Thus its action could be stopped at any desired point. Then the section was stained in Aniline blue and mounted. The protoplasm was contracted, the cell wall partially dissolved, and although the middle lamella still remained it was easy to make out that the processes from one cell to another were optically continuous. I have dwelt on the pulvinus of Mimosa because it affords the best text for such a sermon. I cannot however doubt that the same means of communication exist in organs other

than these and are widely distributed, although I am not at present in a position to support this statement by facts.

I think then it may now be urged with fair certainty that the propagation of the stimulus is effected by means of these protoplasmic threads, and the whole pulvinus may well be compared to an ordinary animal muscle. As I understand, it matters not how fine the connections are as long as they are there. With regard to the cause of the escape of water I cannot but think that there is no doubt that it is caused by contraction. There are two principal views on the subject, those of Pfeffer and those of Vines. The former supposes that by the action of the stimulus the protoplasm undergoes some change by which it becomes suddenly permeable to the cell sap. The water simply runs out. The latter relying, as it seems to me, on the true analogy between animals and plants, suggests that there is a definite contraction and in fact a filtration of water under pressure. According to Pfeffer we have the already omnipotent protoplasm laden with a fresh and certainly unknown power for which we have no grounds for support. We require to simplify matters and diminish the load, not to increase it by fresh untenable hypotheses. The wide bearing of this subject cannot fail to be apparent. We are now in a position if not to explain, at least to get a somewhat clearer insight into such phenomena as the wonderful action of a germinating embryo on the endosperm cells which are practically outside of it, of the various phenomena attending the whole process of germination, of the action of a tendril towards its support, of the action of the root tip with regard to the medium which it traverses and the movements it makes.

Nov. 27, 1882.

MR GLAISHER, PRESIDENT, IN THE CHAIR.

The following communications were made to the Society:

(1) *On the Complex Multiplication of Elliptic Functions.* By A. G. GREENHILL, M.A.

Jacobi, in a posthumous paper, "De multiplicatione functionum ellipticarum per quantitatem imaginariam pro certo quodam modulorum systemate" (*Gesammelte Werke*, Vol. I. p. 491) has shown how, when $\frac{K'}{K} = \sqrt{n}$ and n is an odd integer, to express sn $(a + ib\sqrt{n})u$

in terms of $\operatorname{sn} u$, by means of a transformation of the pth order, where $p = a^2 + nb^2$, and a and b are integers.

Denoting $\operatorname{sn}(a + ib\sqrt{n})u$ by y, and $\operatorname{sn} u$ by x, and putting $\dfrac{cK + iK'}{p} = \omega$, where $c^2 + n$ is a multiple of p, then Jacobi states that

$$y = (a + ib\sqrt{n})\, x \prod_{s=1}^{s=p-1} \frac{1 - \dfrac{x^2}{\operatorname{sn}^2 2s\omega}}{1 - k^2x^2 \operatorname{sn}^2 2s\omega}.$$

It is necessary however to introduce the restriction that a is an odd number, and b an even number: and it is easily seen that this restriction is required in order that $\operatorname{sn}(a + ib\sqrt{n})u$ may be expressed entirely in terms of $\operatorname{sn} u$.

In the other two cases to be taken into account, namely, a even and b odd, and a odd and b odd; it is easily seen that if a is even and b is odd, then $\operatorname{sn}(a + ib\sqrt{n})u$ must have a factor $\operatorname{cn} u$; and that if a is odd and b is odd, then $\operatorname{sn}(a + ib\sqrt{n})u$ must have a factor $\operatorname{dn} u$.

If however we work throughout with the function cn, then these restrictions disappear, as well as for the function dn.

We shall therefore, henceforth, seek to determine $\operatorname{cn}(a + ib\sqrt{n})u$ in terms of $\operatorname{cn} u$. This is equivalent to taking Abel's canonical form of the elliptic integral

$$\int \frac{dx}{\sqrt{\{(1 - x^2)(1 + e^2x^2)\}}},$$

and then $e = \tan\theta$, where θ is the modular angle; or

$$\int \frac{dx}{\sqrt{\{(1 - x^2)(c^2 + x^2)\}}},$$

and then $c = \cot\theta$; instead of taking Jacobi's canonical form

$$\int \frac{dx}{\sqrt{\{(1 - x^2)(1 - k^2x^2)\}}},$$

where $k = \sin\theta$.

Then Abel's function ϕu is equivalent to Jacobi's function $\operatorname{cn}(K - u)$. (Abel, *Œuvres Complètes*, p. 265.)

First, suppose $\dfrac{K'}{K} = \sqrt{3}$; then as we know, the modular angle is 15°, and $c = \cot\theta = \dfrac{k'}{k} = 2 + \sqrt{3}$.

Denoting $\text{cn}\, a$ by x and $\text{cn}\, \omega a$ by y, where $\omega = \cos\frac{2\pi}{3} + i\sin\frac{2\pi}{3}$, an imaginary cube root of unity: then

$$1 - y = P(1-x) \div D,$$
$$1 + y = Q(x+ic) \div D,$$
$$y + ic = R(x-ic) \div D,$$
$$y - ic = S(1+x) \div D;$$

so that y can be expressed in terms of x by means of a linear transformation, viz.

$$y = \frac{\omega(k' + kx) - k'(1-x)}{\omega(k' + kx) + k(1-x)},$$

where $c = 2 + \sqrt{3}$; and P, Q, R, S are determined from the conditions that $x = 1$ and $y = 1$ simultaneously when $a = 0$; or $x = 0$, $y = \sqrt{ic}$ simultaneously when $a = K$.

This linear transformation may be identified with numbers III., IV., V. and VI. of Abel's linear transformations (Abel, *Œuvres Complètes*, t. I., p. 569).

Conversely, these algebraical relations between x and y lead to the differential relation

$$\frac{dy}{\sqrt{\{(1-y^2)(y^2+c^2)\}}} = \frac{\omega\, dx}{\sqrt{\{(1-x^2)(x^2+c^2)\}}}.$$

In the reduction of the integral

$$\int \frac{\phi(z)\, dz}{(z^3-1)\sqrt{(z^3-b^3)}},$$

considered by Legendre (*Fonctions Elliptiques*, t. I. Chap. LXVI.) the modulus of the elliptic functions is $\sin 15°$, and elliptic integrals of the third kind are introduced, in which it may be proved that the parameters, according to Jacobi's notation, are of the form a, ωa, $\omega^2 a$; where $\omega^3 = 1$. (*Quarterly Journal of Mathematics*, Vol. XVIII. p. 66.)

Next, suppose $\frac{K'}{K} = \sqrt{7}$; then, θ denoting the modular angle,

$$\sin 2\theta = \tfrac{1}{8}, \text{ and } c = \frac{k'}{k} = \cot\theta = 8 + 3\sqrt{7},$$

from the modular equation; and if

$$x = \operatorname{sn} a, \quad y = \operatorname{cn} \tfrac{1}{2}(1 + i\sqrt{7})\, a,$$

$$1 - y = P(1 - x)(ic - x) \div D,$$

$$1 + y = Q(\sqrt{ic} - x)^2 \qquad \div D,$$

$$y + ic = R(1 + x)(ic + x) \div D,$$

$$y - ic = S(\sqrt{ic} + x)^2 \qquad \div D;$$

a transformation of y in terms of x of the second order. From the simultaneous values $x = 0$, $y = \sqrt{(-ic)}$, corresponding to $a = K$, it follows that

$$\frac{1-y}{y+ic} = \sqrt{\frac{i}{c}} \cdot \frac{1-x}{1+x} \cdot \frac{ic-x}{ic+x},$$

$$\frac{1+y}{y-ic} = \sqrt{\frac{i}{c}} \cdot \left(\frac{\sqrt{ic}-x}{\sqrt{ic}+x}\right)^2.$$

Conversely these two equations may be proved to be consistent, and to lead to the differential relation

$$\frac{dy}{\sqrt{\{(1-y^2)(y^2+c^2)\}}} = \frac{-\tfrac{1}{2}(1+i\sqrt{7})\,dx}{\sqrt{\{(1-x^2)(x^2+c^2)\}}},$$

where $c = 8 + 3\sqrt{7}$.

I am indebted to Captain P. A. Mac Mahon, R.A., for the discovery of a misprint in Cayley's *Elliptic Functions*, which retarded the verification; and also to Mr Pilkington, Fellow of Pembroke College, for the algebraical verification of this theorem. The misprint is on p. 74, and consists in putting $\operatorname{cn} \tfrac{1}{2}(K + iK') = -\dfrac{1-i}{\sqrt{2}}\sqrt{\dfrac{k'}{k}}$, or $-\sqrt{(-ic)}$, instead of $\sqrt{(-ic)}$, as it should be.

Generally, if $\dfrac{K'}{K} = \sqrt{n}$, where n is of the form $4p - 1$; or $\equiv 3 \pmod 4$; then if

$$x = \operatorname{cn} a, \qquad y = \operatorname{cn} \tfrac{1}{2}(1 + i\sqrt{n})\, a,$$

and

$$\omega = \frac{K - iK'}{p};$$

$$\frac{1-y}{y+ic} = \sqrt{\frac{i}{c}} \prod_{s=0}^{s=p-1} \frac{\operatorname{cn} 2s\omega - x}{\operatorname{cn} 2s\omega + x},$$

and

$$\frac{1+y}{y-ic} = \sqrt{\frac{i}{c}} \prod_{s=0}^{s=p-1} \frac{\operatorname{cn}(2s+1)\,\omega - x}{\operatorname{cn}(2s+1)\,\omega + x},$$

a transformation of the order p.

The form of the transformation may be inferred from the considerations that

(1) when $1-y=0$,

$$\tfrac{1}{2}(1+i\sqrt{n})\,a=2mK+2im'K' \quad (m+m' \text{ even}),$$

$$a=\frac{(m+im'\sqrt{n})(i-i\sqrt{n})}{p}K,$$

$$=\frac{m+m'n}{p}K+\frac{m'-m}{p}iK',$$

$$=4m'K+(m-m')\,\omega,$$

$$=4m'K+2s\omega,$$

and therefore $$x=\operatorname{cn}2s\omega\,;$$

(2) when $1+y=0$,

$$\tfrac{1}{2}(1+i\sqrt{n})\,a=2mK+2im'K' \quad (m+m' \text{ odd}),$$

$$a=4m'K+(m-m')\,\omega,$$

$$=4m'K+(2s+1)\,\omega,$$

and $$x=\operatorname{cn}(2s+1)\,\omega\,;$$

(3) when $y+ic=0$,

$$\tfrac{1}{2}(1+i\sqrt{n})\,a=(2m+1)\,K+(2m'+1)\,iK' \quad (m+m' \text{ even}),$$

$$a=(4m'+2)\,K+(m-m')\,\omega,$$

$$=(4m'+2)\,K+2s\omega,$$

$$x=-\operatorname{cn}2s\omega\,;$$

(4) when $y-ic=0$,

$$\tfrac{1}{2}(1+i\sqrt{n})\,a=(2m+1)\,K+(2m'+1)\,iK' \quad (m+m' \text{ odd}),$$

$$a=(4m'+2)\,K+(2s+1)\,\omega,$$

$$x=-\operatorname{cn}(2s+1)\,\omega.$$

Therefore

$$1-y=P\Pi(\operatorname{cn}2s\omega-x) \qquad \div D,$$
$$1+y=Q\Pi\{\operatorname{cn}(2s+1)\,\omega-x\}\div D,$$
$$y+ic=R\Pi(\operatorname{cn}2s\omega+x) \qquad \div D,$$
$$y-ic=S\Pi\{\operatorname{cn}(2s+1)\,\omega+x\}\div D\,;$$

and P, Q, R, S are determined from the simultaneous values $x=1$ $y=1$ and $x=0$, $y=\sqrt{(-ic)}$.

By a repeated application of the process we can express

$$\operatorname{cn}\{\tfrac{1}{2}(1+i\sqrt{n})\}^t a$$

in terms of $\operatorname{cn} a$ by a transformation of the order p^t.

The case of $n=11$ presents peculiar difficulties; we therefore pass on to the case of $\dfrac{K'}{K}=\sqrt{15}$, then $\sqrt[4]{(kk')}=\sin 18^\circ$ (Joubert, *Comptes Rendus*, t. 50);

and
$$c=(7+4\sqrt{3})(4+\sqrt{15}),$$

$$\sqrt{(2c)}=(2+\sqrt{3})(\sqrt{5}+\sqrt{3}).$$

If
$$\frac{1-y}{y+ic}=\sqrt{\frac{i}{c}}\cdot\frac{1-x}{1+x}\cdot\frac{ic-x}{ic+x}\left(\frac{\sqrt{ic}-x}{\sqrt{ic}+x}\right)^2,$$

then
$$\frac{1+y}{y-ic}=\sqrt{\frac{i}{c}}\left(\frac{\alpha-x}{\alpha+x}\right)^2\left(\frac{\beta-x}{\beta+x}\right)^2,$$

where $\alpha=\operatorname{cn}\frac{1}{4}(K-iK')$, $\beta=\operatorname{cn}\frac{3}{4}(K-iK')$; and therefore $\alpha\beta=ic$; also

$$\frac{dy}{\sqrt{\{(1-y^2)(y^2+c^2)\}}}=\frac{-\frac{1}{2}(1+i\sqrt{15})\,dx}{\sqrt{\{(1-x^2)(x^2+c^2)\}}}.$$

These results have also been verified algebraically by Mr Pilkington.

Returning to the case where $\dfrac{K'}{K}=\sqrt{5}$, then

$$2kk'=\sqrt{5}-2=\left(\frac{\sqrt{5}-1}{2}\right)^3,$$

$$c=\sqrt{5}+2+2\sqrt{(\sqrt{5}+2)},$$

$$\sqrt{c}=\frac{\sqrt{5}+1}{2}+\sqrt{\left(\frac{\sqrt{5}+1}{2}\right)}$$

(Abel, *Œuvres*, p. 382).

It is not possible to express $\operatorname{cn}\frac{1}{2}(1+i\sqrt{5})\,a$ rationally in terms of $\operatorname{cn} a$; but if $x=\operatorname{cn} a$, $y=\operatorname{cn}(1+i\sqrt{5})\,a$, then y can be expressed in terms of x by the sextic transformation

$$\frac{1-y}{1+y}=M\frac{1-x^2}{x^2+c^2}\left\{\frac{\operatorname{cn}^2\frac{2}{3}(K-iK')-x^2}{\operatorname{cn}^2\frac{1}{3}(K-iK')-x^2}\right\}^2,$$

or
$$\frac{y+ic}{y-ic}=Nx^2\left\{\frac{\operatorname{cn}^2\frac{1}{3}(K+2iK')-x^2}{\operatorname{cn}^2\frac{1}{3}(2K+iK')-x^2}\right\}^2.$$

We had $\frac{K'}{K}=\sqrt{3}$, and a, ωa, $\omega^2 a$ the parameters of the elliptic functions of the third kind in the reduction of the integral

$$\int \frac{\phi(z)\,dz}{(z^3-1)\sqrt{(z^3-b^3)}};$$

and it will be an interesting subject of enquiry to determine the simplest form of the integrals which reduce to elliptic integrals of the third kind in which

$$\frac{K'}{K}=\sqrt{5},\ \sqrt{7},\ \sqrt{15},\ldots$$

and the parameters involve the corresponding complex multipliers.

(2) *On certain points in the function of the cardiac muscle.* By W. H. Gaskell, M.D., F.R.S.

Part I. *On the rhythmical properties of the cardiac muscle.*

In a former paper[1] I have described experiments which brought me to the conclusion that the beat of the frog's heart was dependent on separate impulses proceeding from the sinus to the auricles and ventricle and travelling along distinct nerve paths. Also, the modifications of the sequence of ventricular upon auricular beat caused by various methods of procedure, such as tightening the clamp between auricle and ventricle, heating the auricles and sinus, the application of various poisons &c., were all discussed on the supposition that a contraction can only occur when a certain relation exists between the strength of the impulse and the excitability of the muscular tissue.

In a subsequent paper[2] I have shown by experiments upon the heart of the tortoise that the conception of separate impulses passing from the sinus to the auricles and ventricle is unnecessary, and that all the variations of sequence observed in the case of the frog as well as those noticed in the tortoise, are to be explained by the more or less complete blocking of a contraction-wave, which starting at the sinus passes over the auricles and when it reaches the ventricle causes a contraction there. I still however imagined that the mediation of some nervous apparatus such as ganglion cells was involved in the transference of the contraction from auricle to ventricle.

Finally in a paper[3] read before the British Medical Association last August, I showed that no need exists for the intervention of

[1] *Phil. Trans.* 1882. [2] *Journ. Physiol.* Vol. III. p. 369.
[3] *British Med. Journ.* 1882, p. 572.

special ganglionic apparatus to conduct the contraction from sinus to auricle or from auricle to ventricle, for in both the frog and the tortoise a band of circularly disposed muscular fibres exists at the sino-auricular and auriculo-ventricular junctions, through which the contraction wave is able to pass from sinus to auricle and from auricle to ventricle respectively.

Further the resemblance between the artificial blocking of the contraction wave by means of section to the natural block at the sino-auricular and auriculo-ventricular junctions was pointed out, and it was shown that the pauses between the contractions of the several cavities of the heart could be ascribed to the nature of the muscular tissue at these junctions together with the anatomical arrangement of the fibres there.

The ultimate conclusions therefore obtained from those parts of these three papers which dealt with the beat of the heart are the following:

1. The beats of the heart represent a series of separate peristaltic contractions which start from the sinus end and pass in regular order over the whole heart.

2. The peristaltic nature of these contractions is obscured by the fact that the wave of contraction passes along a muscular tube which is not of the same diameter or structure throughout; so that the contractions of certain portions, which by bulging have become more prominent and by a higher development of structure have become more rapid in their contraction, are so pronounced as to give the impression of separate contractions of these portions with pauses between them.

3. The conduction of this wave of contraction can be artificially hindered by various methods in any part of its course; so that if the hindrance is but slight an artificial pause is produced, if greater a partial block, in which case every second contraction passes the block, if still greater an increasing block until at last the block is complete and no contraction-wave is able to pass.

So far I have but slightly touched upon one most important point in this conception of the heart's rhythm, viz. the nature of the starting point of this contraction-wave. It is the consideration of this point to which I desire to call attention in this part of the present paper.

In the frog the septum between the two auricles plays an important rôle in all discussions upon the nature of the rhythmical contractions which occur after the removal of the sinus. In the tortoise on the other hand the septum possesses no such importance, its place is taken by a band of tissue, which passing from the sinus to the ventricle connects the two auricles together and is the path along which the coronary veins and the nerve trunks

with their accompanying ganglia pass between the sinus and the ventricle. The thin inter-auricular septum arises along the median line of this—so to speak—unbulged portion of the auricles. The muscular tissue of this portion differs from that of the rest of the auricles in not possessing any loosely reticulated structure, its fibres being arranged closely together parallel to those of the sino-auricular and auriculo-ventricular muscular rings. In fact this band of tissue is exactly what would result if in an original muscular tube containing nerve fibres and blood-vessels, in which the muscle fibres were arranged circularly, a bulging of the muscular tissues were to occur at one place which finally reached such an extent as to flatten out the rest of the tube.

This band of tissue with the auriculo-ventricular ring at the one end and the sino-auricular at the other possesses rhythmical properties next in importance to those of the sinus, and owing to its external position it is easy by means of consecutive sections or by clamping at different places to compare the rhythmical property of one portion with that of another.

In all cases the method of suspension as described in my former papers was used, the fixed point being obtained either by means of clamping or by holding the aorta firmly. In this way all the different experiments are conducted under much the same conditions.

It is possible by means of the clamp or by section to remove the sinus and adjacent parts without causing such an excitation as is usually seen in the case of the second Stannius ligature, so that the investigation need not be obscured by the production of a series of beats which are simply caused by excitation and are not spontaneous. In such cases we can compare efficiently the nature of the spontaneous rhythm of auricle and ventricle when the sinus only is removed, of ventricle and half the auricle when the section is made in the middle of the inter-auricular band of tissue already mentioned and of ventricle alone when both sinus and auricles are removed. In all three cases the resulting phenomena are the same in kind and vary only in degree. In all three cases a longer or shorter standstill may occur before spontaneous rhythm commences. Under the same conditions however the length of this standstill is as a rule greater the further away from the sinus the section is made.

Also the spontaneous rhythm usually commences with beats at long intervals from each other which steadily and gradually quicken, until a certain definite regular rate is attained, which continues for a long period without much alteration increasing rather than diminishing in the course of time. A marked contrast is thus presented to the so-called rhythmical beats which are seen in the frog's ventricle upon the application of the second

Stannius ligature, or in either frog or tortoise when the clamp in the auriculo-ventricular groove is suddenly tightened, for such contractions always commence with a quick rate and become slower and slower until in a short time they disappear. Again just as we have seen that the spontaneous rhythm is set up quicker the nearer the section is to the sinus, so also is the maximum rate attained more quickly, and the rapidity reached more nearly approaches that of the sinus itself the nearer the section is to the sinus.

Thus the manifestation of the spontaneous beats of the sinus, of the whole auricle, and of the whole ventricle presents a descending series of phenomena in which the connection between two consecutive links is so close, as to lead to the conclusion that the same cause underlies the rhythmical spontaneity in each case. If then the beats of the sinus and therefore of the heart as a whole are due to motor ganglia, so too are the spontaneous contractions of the isolated ventricle; if on the other hand, reason is given to ascribe the ventricular beats to a rhythmical property inherent in the ventricular muscle, then it is a fair conclusion to attribute the sinus beats and therefore the heart beat to a similar property of the muscular tissue of the sinus. That the latter of these two views is the correct one the following experiment seems to show.

Two strips of tissue are cut from the apex of the ventricle in the same direction, of the same length and are in every respect as similar as possible to each other; these two pieces are then suspended in two moist chambers and their movements registered by a lever attached to the free end of each strip by means of a thread. The one preparation is left entirely alone, the other is stimulated every ten seconds by single induction shocks applied at the fixed end, and at intervals a weak interrupted current is sent through the whole strip by means of two thin wire electrodes which are so fine and light as not in any way to hinder the movements of the muscle.

It is then found that after about an hour and a half or slightly longer the single induction shocks are no longer required in order to induce rhythmical contractions of the strip of muscle, for the strip is now contracting of itself with increasing regularity of rhythm and with strong vigorous contractions, while the control strip which has been left alone still remains absolutely quiescent. The effect then of the single induction shocks combined with the weak interrupted current has been *to bring the muscular strip into such a condition of instability that rhythmical discharges take place in it, so causing rhythmical contractions.* These rhythmical beats are clearly not simply the after effect of the previous stimulations in the same way, as a stimulus applied to the base of the frog's ventricle produces not a single beat but a series

of contractions, for, when the strip has once been brought into this condition of spontaneous rhythm, it will continue beating for very many hours. Thus I have induced spontaneous rhythm in this way and have found the piece still contracting rhythmically and well 30 *hours* after suspension, i.e. between 28 and 29 hours after the commencement of the rhythm.

Also the control piece which is left entirely to itself will after a time begin to contract rhythmically so that the power of rhythmical contraction exists in both strips; *the effect therefore of the previous stimulation is not to invest the tissue with a property which it did not possess before, but to quicken the establishment of certain conditions in the muscular substance upon which the manifestation of that property depends.* As far as I have yet seen the beneficial effects of previous stimulation are sufficient to shorten the time by one-half; the strip which is left to itself does not begin to beat until nearly four hours after suspension, whereas the stimulated piece beats between one and two hours; in addition the contractions of the latter are more vigorous, the rhythm is more regular and quicker.

Here again, then, experiment points to a difference of degree rather than of kind between the rhythm of the whole ventricle and that of the isolated strip from the apex. In each case the rhythmical contractions take time to be manifested, the difference being in the length of time; in each case the rhythm is at first slow and gradually quickens up to a definite rate and in each case the rhythm when once established is practically permanent. The duration of the preliminary standstill may be most markedly shortened by sending a weak interrupted current and single induction shocks through the apex strip; while in the whole ventricle it may be removed altogether by sending a supply of blood through the coronary system. In the apex the previous stimulation causes the resulting spontaneous rhythm to be more regular and quicker; in the whole ventricle the rate of the spontaneous rhythm depends directly upon the pressure in the coronary blood-vessels.

Further, this rhythm is clearly due to the muscle itself, for ganglion cells are not to be found in this strip, no foreign blood is used as in the ordinary method of obtaining rhythmical apex contractions, and the mechanical stimulus of the stretching of the tissue in consequence of the method used is not sufficient to explain the phenomenon in question, for, as soon as the rhythm is well established, the strip can be removed, laid down and kept moist, and will still continue to beat rhythmically in this position for hours; nay more, it can be cut into pieces and each piece will beat just as though, according to the accepted theory, it were provided with ganglia. In the same way a strip of the muscular

tissue of the auricle of the tortoise can be taught to beat rhythmically.

I conclude, then, that every part cf the muscular tissue of the tortoise heart possesses the property of spontaneous rhythmical contraction, and that the difference in function between the muscular tissue of one part and that of another depends upon the extent to which that property has become on the one hand specially developed or on the other hand rudimentary, owing to the greater development of some other property such as rapidity of contraction, which was more advantageous for the efficiency of the heart as a whole.

In the muscular tissue of the frog's ventricle this rhythmical property seems to be still more rudimentary than in the tortoise; I have not yet succeeded in teaching a strip from the apex of the frog's ventricle to beat spontaneously, though the experiments of Merunowicz and others are sufficient to show how little extra assistance is needed to bring about the required condition of the muscle.

In conclusion, I will put the question: Is not the power of spontaneous rhythmical contraction a fundamental property of all contractile tissue, a property which has in some cases developed so as to become the chief attribute of the muscle in question, in other cases has fallen more or less into the background, become more or less rudimentary according as the muscle has specialized in other directions, such as the power of rapid contraction, tonic contraction, etc.? To discuss this question at full length would take up too much time, I will only say that recent investigations have shown that in all three kinds of muscular tissue unstriped, cardiac, and striped (especially in more lowly developed animals), it is possible in different ways, apart from rhythmical stimulation, to throw the tissue into such a condition that rhythmical contractions result, even when no signs of ganglion cells are to be found.

PART II.—*On the action of a weak interrupted current upon certain functions of the cardiac muscle, and its resemblance to the action of the vagus nerve.*

In my first paper[1] on the action of the vagus nerve, I have shown that stimulation of the vagus in the frog produces throughout pairs of opposite effects which are not dependent on each other, but may each one occur separately. Thus we may have

1. Slowing and acceleration of rhythm,
2. Diminution and increase of the force of the contractions,
3. Diminution and possibly increase of tone.

[1] *Phil. Trans.* 1882.

Besides these three sets of effects I came to the conclusion that a fourth must be added; viz. diminution and increase of excitability; since then, however, my experiments on the tortoise heart[1] have shown me that the observations upon which I relied for the proof of the influence of the vagus on the muscular excitability were in reality proofs of the action of the nerve upon the power of the muscular tissue to conduct a wave of contraction along it. This is clearly shown by the resemblance between the results of these experiments on the frog and those described in the two subsequent papers quoted above, which demonstrate conclusively the power possessed by the vagus of removing and increasing a block of the contraction-wave caused artificially by the section of the auricle of the tortoise. We may therefore add to the above mentioned effects of vagus action

4. Diminution and increase of conduction power.

Further, in close connection with this last action is the influence of the vagus on the sequence of the contractions; thus, as is pointed out in my paper read before the British Medical Association, the natural slight blocking of the contraction-wave at the junctions of the sinus and auricles and of auricles and ventricle respectively, may be converted by the vagus into a complete block, so that the contraction-wave may never reach the auricle in the one case or the ventricle in the other. On the other hand when the natural block at these two junctions is by any means intensified so that only every second contraction-wave passes, then the vagus may remove that block and cause every contraction to pass.

Again the length of the pause between the contractions of the sinus and the auricle may be increased or diminished by the action of the vagus; this is especially well seen in the snake when as is apt to occur a very measurable pause ensues between the contractions of the sinus and auricles. We may therefore add as corollaries to (4)

5. Cessation and recovery of sequence,

6. Diminution and increase in rapidity of sequence.

All these different effects may on the view put forth in Part I. of this paper be summed up by saying,

The vagus possesses the power of depressing and exalting all the different functions of all the different muscular tissues of the heart.

In addition, it is to be noticed that although the initial effect of the vagus is often to depress some function, its final and most enduring power is to exalt, intensify and repair that function. Thus, although it slows rhythm, yet its stimulation makes the rhythmical power last longer than it otherwise would, and makes

[1] *Op. cit.*

the heart beat with regularity when it was previously irregular; although it reduces the force of the contractions, yet its ultimate effect is to improve and sustain the contraction force; although it may diminish the conduction power yet in the end it completely repairs that power.

The vagus then is essentially the trophic nerve of the heart.

In endeavouring to proceed further and to find out the nature of this trophic action, it is necessary first to draw attention to certain differences in the behaviour of the hearts of the frog and the tortoise when the vagus nerve is stimulated.

In the frog standstill of the auricles and ventricle may be caused by the vagus in three ways,

1. By cessation of the rhythm.
2. By diminution of the contractions to the null point, without affecting the rhythm.
3. By the prevention of the passage of the contraction-wave from the sinus to the auricles.

In the tortoise, standstill is produced by the first and third methods, never as far as I have seen by the second.

In the frog, the force of the contractions of both auricles and ventricle are affected in a similar manner. In the tortoise the ventricular contractions are absolutely unaffected, while the auricular resemble those of the frog in their behaviour to vagus stimulation.

This absolute indifference displayed by the ventricle of the tortoise to the influence of the vagus is so striking and unexpected that I have especially endeavoured to find out the meaning of it. Thus it was possible that the coronary system possessed by the ventricle might play the chief part in regulating the force of the ventricular contractions and that the vagus might act on that system.

I therefore left the heart in the body with blood flow intact, and in the same way as in the suspended heart fixed a point between auricles and ventricle by fastening the outer surface of the base of the aorta to a rigid vertical rod, a ligature attached to the apex of the ventricle was made by a simple contrivance to move a lever up and down without disturbing the natural position of the heart. I found, however, in this case as well as in the case of the suspended empty heart, that the force of the ventricular contractions was not influenced by the vagus. In addition to this experiment I caused an artificial blood solution to flow steadily through the coronary system by means of a cannula fixed in the aorta, but with no better success; all I found was, that the force of the contractions varied directly as the pressure in the coronary vessels, but there was no evidence that the vagus possessed any influence on that force.

After these preliminary observations we can turn our attention to the strip of muscular tissue cut from the apex of ventricle or auricle as mentioned in Part I. of this paper. In the first place, it is of great interest to observe the way in which the interrupted current brings the muscle into the condition of rhythmical spontaneity. The result of cutting out the strip is to produce in it a series of blocking points some of which are complete, some partial, so that each contraction-wave at first travels a little way, is then at one place completely blocked, at another delayed in its progress, at another partially blocked, so that every second wave passes and so on; the total result being to produce contractions of irregular form and irregular force. The effect of the weak interrupted current is gradually and slowly to remove these separate blocks, so that the contractions caused by the single induction shocks gradually become stronger, the irregularities disappear and the alternately strong and weak contractions become all strong. Finally, when the blocks have been all removed, when the contraction-wave travels freely from one end of the strip to the other then the process of repair is complete, then spontaneous contractions appear, and the strip can now be left to itself and will continue beating as long as I have had the patience to watch it.

We see then that an interrupted current sent through the strip of muscle, which is not strong enough to cause contractions in that strip, repairs the conduction power of the muscular vagus and in this respect therefore acts very similarly to the tissue nerve.

Again, a weak interrupted current sent through a strip of muscle from the apex of the frog's ventricle or the auricle of the tortoise, when they are made to beat regularly by means of single induction shocks sent in every five seconds, diminishes most markedly the force of these artificial beats during the stimulation and causes an increase after the end of the stimulation exactly similar to what is seen when the vagus is stimulated in the whole heart.

Further, atropin applied to the muscular strip prevents this action of the interrupted current in exactly the same way as it prevents the vagus action.

On the other hand, if the interrupted current be sent through the strip from the ventricle of the tortoise, when that tissue has recovered its conduction power, or when it is beating spontaneously, no effect whatever or only the very slightest is produced upon the force of the contractions either during or after the stimulation. In this case too then the weak interrupted current closely resembles the action of the vagus.

It is at present premature to speculate upon the manner in which an interrupted current, not strong enough to cause a contraction, is able to produce these effects in an isolated strip of

cardiac muscle; and it is of course possible to say, that in reality the current acts by stimulating the endings of the vagus in the muscle substance and not directly upon the muscle itself. Unfortunately in the case of the cardiac muscle this question cannot be decided by the use of curare, yet for other reasons I am inclined to think that both the interrupted current and the atropin produce the effects observed by direct action upon the muscle substance itself. The following consideration especially seems to justify this view.

If the interrupted current in reality stimulates vagus fibres, then also the single induction shocks must stimulate those same fibres, and therefore the series of single induction shocks ought to produce the same effect upon the muscular strip, as a series of single induction shocks applied to the vagus nerve would produce upon the muscular tissue of the auricle or ventricle respectively. Now single induction shocks applied every five seconds to the strip from the auricle of the tortoise produce regular contractions, which increase to their maximum height and then remain fairly constant at that height; there is no sign of any diminution of force such as is immediately manifested when the interrupted current is sent through the strip. On the other hand, no better stimulus can be desired for the vagus nerve than a series of single induction shocks every five seconds; the auricular contractions are immediately and very greatly reduced in force, and can be kept small for a long time; nay more, one single induction shock applied to the vagus is sufficient to reduce most markedly the next five or six auricular contractions.

Seeing then that the vagus proves itself most sensitive to the stimulus of a series of single induction shocks, while the strip of muscle behaves either with indifference or in exactly the opposite direction to the same kind of stimulation, it seems to me more near the truth to say that the action of the vagus nerve resembles that of a weak interrupted current applied to the muscle itself, than to argue that the interrupted current produces the effect observed because it stimulates vagus fibres in the strip of muscle experimented on. If this view be correct, then the question naturally arises, how is it that the vagus possesses only trophic functions and not motor ones, for the interrupted current if strong enough becomes motor in its action? A question which leads the imagination to wonder whether the presence of ganglion cells in the course of the nerve fibres may prove to be the reason why the vagus is not a motor nerve.

(3) *On the Development of the Pollinium in Asclepias.* By THOS. H. CORRY, B.A.

So far as I have been able to ascertain no observer has fully investigated, in an adequate manner the whole mode of formation of the pollen-mass or *pollinium* in this genus, and in the natural order to which it belongs. Hofmeister[1] and Schacht[2] alone have thrown some light upon its history in the early stages with however somewhat contradictory results, while Schleiden's[3] account of it is incorrect in several respects and very fragmentary. Francis Bauer, Ehrenberg[4], Robt. Brown[5], Adolphe Brongniart[6], and the younger Reichenbach[7] have all recorded details more or less exact concerning its structure but principally in some of the later stages when the flower is becoming rapidly mature.

In the very young anther, which has the form of a very slightly flattened spatula with a strongly convex dorsal surface, I was able to trace in transverse section that a single cell of the hypodermal row, lying laterally but towards the internal side of each lobe of the anther, and containing granular protoplasm and a prominent nucleus, had undergone longitudinal division, parallel to the long axis of the anther. This hypodermal cell in which division occurs constitutes the *archesporium* of Goebel[8]. The archesporium so divided consists therefore of an inner and an outer segment. The

[1] Zur Entwickelungsgeschichte der Zostera, *Bot. Zeitung*, 1852, No. 7, pp. 121—131, plate iii.

[2] *Das Microskop*, ii. Aufl. p. 166 et seq.

[3] *Grundzüge.*

[4] *Linnæa*, IV. p. 94, 1829; also *Trans. Royal Acad. of Sciences, Berlin*, Nov. 1831.

[5] *Linn. Trans.*, Vol. XVI., p. 717 et seq. 1833.

[6] *Ann. des Sci. Naturelles*, Ser. I., Vol. XXIV., pp. 263—279, plates 13—14 B.

[7] *De Pollinis Orchidearum Genesi ac Structura*, Leipzic, 1852.

[8] Beiträge zur vergleichenden Entwickelungsgeschichte der Sporangien. *Bot. Zeit.* 1881.

inner segment in each lobe is in reality the *primary-mother-cell of the pollen.* Each primary-mother-cell, as seen in transverse section, will be found when viewed longitudinally to correspond to a single longitudinal row of somewhat cubical cells rather higher however than they are broad or long. Since only a single row of primary-mother-cells is formed in each lobe the anther is bilocular from the beginning and never at any period quadrilocular. The outer or more superficial segment of the primitive archesporial cell then becomes successively divided longitudinally in a tangential plane in such a manner that in transverse section three layers of cells are now apparent. In these latter radial, horizontal, and further tangential divisions successively occur. The cells which constitute the innermost of these three layers form by radial division a peculiar epithelium of rectangular cells investing on the inner aspect the primary-mother-cell: this is the *tapetum.* The cells of the tapetum *proper* are reinforced by a corresponding layer on the external side of the anther-lobe, formed by means of a series of internal segments cut off vertically from the cells of the parenchymatous ground tissue in that region. In this manner a limiting membrane is formed which entirely surrounds and invests on all sides the primary-mother-cell of the pollen.

The tapetal cells *proper* are thus derived from a portion of the primitive archesporium, while those cells by which the layer is completed towards the outer side of the anther, and which appear in transverse section to be longer and more oblong than the real tapetal cells, are not so derived. Each cell of the limiting membrane contains a prominent nucleus surrounded by granular protoplasm.

The primary-mother-cell of the pollen is when viewed in transverse section at first somewhat hexagonal in shape, single and of relatively large size, for while the outer segment of the archesporial cell has continued to undergo division, no further division has taken place in the inner segment which has very granular protoplasmic contents and a distinct nucleus.

Very soon however it may be found exhibiting two nuclei produced by the division of the single one, and this nuclear division is speedily followed by division of the protoplasm into two portions, and formation of a longitudinal septum in a direction somewhat oblique to the surface of the anther. Each of these two cells now begins distinctly to elongate in a direction perpendicular to the surface of the anther, and by virtue of this elongation they become very sharply differentiated from the surrounding tissue. As the loculus expands by growth of its walls, this elongation becomes more and more pronounced. Immediately after this change has become fairly well marked, each of the two cells becomes divided by longitudinal division parallel to its long axis, and this is followed by a

transverse, i. e. horizontal, division likewise in the same plane. The cells in consequence of this take the form of short prisms whose direction is inclined obliquely downwards from the surface. Of these prisms four appear in transverse section and the free faces of the two lateral ones are somewhat rounded, so that the whole mass has now a slightly elliptic form. Further division of each of these prisms is then continued in both the longitudinal and horizontal planes parallel to the long axis of the cell; the result of which is that the loculus of the anther now contains a large group of cells comparatively narrow in proportion to their length, which appear in any transverse section as a single row consisting of eight to twelve or more cells. Seen in this view they are of extreme length, being six to ten times longer than they are broad, of large size and rhomboidal or prismatic form, while they pursue a slightly oblique direction; each possesses very granular protoplasmic contents containing a large circular nucleus with a nucleolus, a large vacuole at either end, and a thin cellulose cell-wall. Their vertical or longitudinal walls form a common partition between these cells on the one hand, and the cells of the tapetal limiting membrane which closely surrounds them on the other. Some of them may at a slightly later period be found exhibiting two nuclei in close proximity to each other. In longitudinal sections they may be seen to lie in numerous obliquely directed rows arranged one above the other. But all the narrow prismatic cells contained in a loculus remain parallel and closely appressed together in close and intimate connection one with another so that they cannot be separated one from the other without injury and rupture; in the relative thickness of their walls moreover they present no difference which would enable one to assert with any degree of certainty, when this stage has been reached, that any special aggregation of cells was the direct derivative of one of the segments of the primitive-mother-cell. The coherent tissue completely filling the cavity of the loculus bounded by the tapetal membrane, has throughout thoroughly the appearance of a cell-mass all of whose cells have been repeatedly bisected in succession by a series of divisions in two planes only.

At this stage of their development they correspond exactly to the contents of a single loculus in the young anther of Zostera, a genus of Monocotyledons, whose mode of pollen-formation has been studied in a most masterly manner by Hofmeister[1]. Indeed, the earlier stages of Asclepias and those of this last-named genus exhibit an extremely close correspondence with one another, the only marked difference between the two being that in Zostera the anther is quadrilocular. My observations up to this point

[1] loc. cit. pp. 125—128, plate iii, figs. 4—15 b; also *Neue Beiträge*, ii., pp. 643—645.

accord at first with the single recorded observation of Schacht rather than with those of Hofmeister, though they commence at a much earlier stage than was noticed by either of these writers. Hofmeister regards the pollen as derived not from a *single* primitive-mother-cell, as seen in tranverse section, but from a *group* of primitive-mother-cells. Being unable to trace his 'group' of cells back any farther he regards Schacht's statement and figure[1] that in Asclepias only a *single* primary-mother-cell is formed in each anther lobe as erroneous. The ultimate conclusion reached, if his observations on this point be accepted, is of course a multicellular archesporium; while my own results distinctly prove that it is unicellular, and that Schacht's statement really represents the true condition of the case. Apart from this, however, I have been able completely to confirm Hofmeister's researches in so far as they relate to the pollen-development of Asclepias up to the stage at which the most obvious resemblance to that of Zostera is exhibited.

The cell-walls of the primary-mother-cell and its derivatives by division are thin and always remain so, never being visibly thickened at any subsequent period; in this feature they resemble so far as is known only Zostera and its near ally, Naias[2], while they differ in it from the rest of Angiosperms at large. At this point however, the close analogy to, and correspondence with, the type of pollen-formation in Zostera ends. For in the latter the granular protoplasmic contents of each of these long narrow prismatic cells becomes surrounded by an exceedingly thin and delicate, but readily observable, cellulose membrane, and forms an elongated club-shaped or fusiform pollen-grain, exhibiting therefore perhaps the most primitive type of pollen formation known in the Phanerogams. In Asclepias, on the contrary, further division of each of the prismatic cells takes place resulting ultimately in the formation of the special-mother-cells of the pollen in the following manner.

Succeeding the division of the nucleus of each of the prismatic cell into two parts (which feature it has been already mentioned was observable in some of the cells), the protoplasmic contents now divide vertically into two at right angles to the long axis of the cell, and therefore in the direction of the breadth of the anther and at right angles to all the previous planes of division; simultaneously the formation of a cell-wall takes place in the plane of division, i. e. parallel to the short sides of the prism. By means of this septum the prismatic cell becomes divided into

[1] *Das Microskop*, pl. iii. fig. 8; English Edition, p. 105, fig. 21 a.

[2] W. Hofmeister, *Neue Beiträge zur Kenntniss der Embryobildung der Phanerogamen*, part ii., *Monokotyledonen*, pp. 642—643, plate i., figs. 1—12, 1859.

two smaller segments of oblong form and equal size[1]. The conspicuous nucleus of each of these oblong cells then becomes further subdivided by vertical division at right angles to the length of the cell. This division is followed by division of the protoplasm and formation of a cellulose septum running in the same plane. The walls formed by the two last series of divisions are of course only visible in transverse sections. In the upper narrower part of the anther lobe the number of longitudinal divisions which the primary-mother-cell undergoes is very small, and in consequence of this, fewer long narrow prismatic cells are visible in a transverse section through this part. Further, in this portion sometimes only one of the two oblong cells formed by vertical division of the narrow prism divides again vertically, so that in transverse section three cells only are apparent in an oblique row, viz. one larger and two slightly smaller. The cells formed by these successive vertical divisions of the narrow prisms, each with a conspicuous nucleus, are at first cubical, and in longitudinal section they are seen to be disposed in numerous rows more or less horizontally arranged one above another. Soon, however, they become spherical in form, owing to the rounding off of their walls on all sides, though they still remain firmly adherent together, and at the points where they touch adjoining cells there still exists only a common partition wall. They are the *special-mother-cells of the pollen.* At this period the mass of granular protoplasm contained in each of them cannot be discovered to have any special cellulose coat or wall deposited on it, but is surrounded only by the wall of the special-mother-cell. Thus none of the cell-walls so far produced in the whole course of the development of the pollen undergo absorption as is commonly the case, and as Reichenbach has shewn to be the case in the waxy pollinia of the Orchids (where the mother-cells are broken down and form a viscid pulp in which the tetrads lie)[2], but persist; the cells which they bound, though now become rounded, adhering, as has just been mentioned, closely to one another. Their contents also never become now or subsequently set free except on the rupture and bursting of the pollinium. By the unequal extension of the whole loculus the special-mother-cells contained in it now become polyhedral. They are formed by division of the single primitive-mother-cell in three planes at right angles to each other, but the rhythm of the divisions is quite unique and is not that usually characteristic of Dicotyledons.

[1] The effect of all these divisions in the primary-mother-cell is merely to increase the number of mother-cells from which the special-mother-cells are subsequently to be derived.

[2] Schleiden (loc. cit.) states that the walls of the primary-mother-cells in Asclepias are absorbed, and that at a very early period. Such, however, is not the case. In Naias, according to Hofmeister, they are resolved, along with those of the special-mother-cells.

That vertical wall of each of the limiting tapetal cells which is adjacent to the special-mother-cells now undergoes, at least in part conversion into cutin, and in so doing increases considerably in volume; the chemical change is likewise accompanied by a change in color from colorless to pale yellow. This change is followed successively by a like conversion of all the walls surrounding the special-mother-cells which assume the same tint. On treating these walls with concentrated sulphuric acid a pale ruby-red color is produced in all alike.

In this manner the pollinium is produced, and it can at this period be extracted from the anther-loculus in the form of a single definite compact solid coherent mass of considerable size, with a deep golden-yellow color and a waxy look externally. Its surface, which is perfectly smooth, presents the appearance of being divided in a reticulate manner into areolæ or hexagonal meshes, the apparent bulging of each areola being caused by the shape of the underlying cell filled with protoplasm.

Each pollinium contains all the adherent or firmly united special-pollen-mother-cells produced in one anther-loculus or pollen-sac. In transverse section it exhibits a cellular appearance and structure, consisting of three series or rows of cells[1], parallel to its sides, the middle series being more or less interrupted. These cells are enclosed by thick, pale-yellow-colored, semi-transparent cell-walls, the cell-walls of those belonging to the two outer rows being continuous at certain points with, and surrounded by, a deep golden-yellow, pellucid, cuticularized membrane, which has a resistant horny texture, cuts with great ease, and is derived from the change of those portions of the surfaces of the tapetal cell-membranes immediately adjacent to the special-mother-cells. This membrane, forming an unbroken sheet, encloses and envelops completely every part of the entire, compact, solid, concrete mass of coherent special-mother-cells filling the anther-loculus, thus forming a general coat of considerable thickness. Brongniart[2], and Schleiden (*Principles of Scientific Botany*, Ed. III. 1849, p. 356) both believed that this yellow investing membrane, which I have shewn to be formed from the tapetum was itself really of a cellular nature, i.e. composed of cells; for the former observer tells us that the areolate appearance is due "not to the underlying cellular mass but to the cells themselves, which constitute the membrane and which are disposed after the fashion of epidermal cells;" while the latter regards it as formed "of the outermost layer of the special-mother-

[1] In the oblique planes of the original prismatic mother-cells each row consists of four cells and not three. The relation of the descriptions framed from the two points of view, viz. perpendicular and oblique, is however easily obvious.

[2] loc. cit. p. 267.

cells in which no pollen-grains are developed." A very slight examination will easily afford convincing proof that both of these views are at variance with the facts.

No other observer with the exception of Payer has attempted to fathom the mode of origin of this membrane, and this observer held that the viscid gum forming the "appendages" or "processes" of the stigmatic corpusculum, which he believed was a liquid secreted by a gland (the corpusculum) flowing in lateral channels or grooves, when it arrived at the anther-lobes on which the lateral grooves abut, penetrated into the interior of these lobes and agglomerated the grains of pollen, uniting them afterwards through their whole extent (vide *Traité d'Organogénie comparée de la Fleur*, Vol. I., page 569). This latter investigator did not examine the method of development of the pollinium by means of sections, or it would have been clearly evident to him that the investing membrane is formed and completed at a period long prior to the dehiscence of the walls of anther-loculi and consequent exposure of the pollinia. Also that the only function performed by the corpuscular appendages, when the anther-loculi have opened by dehiscence, is that of firmly attaching the pollinia to their free ends; the substance of the two bodies though externally united being never confounded, but always remaining completely distinct, and moreover giving different reactions with micro-chemical reagents. Schacht believed, on what evidence he does not state, that the investing membrane was "of the nature of a secretion," and this is the view held by Prof. Oliver[1]: but such is certainly not the case; and Dr Maxwell T. Masters in his Article "Asclepiadeæ," (in Lindley and Moore's *Treasury of Botany*,) hazards the statement that it is derived "from the separable inner lining of the anther-cell," probably referring to Brongniart's view above cited.

It is at once obvious that the pollen-grains which are subsequently formed in the special-mother-cells so enclosed cannot be dispersed in the ordinary way, nor can the pollinia fall out of the open anthers spontaneously, but remain seated there so that pollination without foreign aid is impossible; and moreover the flower is so very peculiarly contrived and adapted for the visits of insects in search of honey that the pollinia are by their agency extracted and removed *en masse* from their place of origin and applied by the same medium to a distant part of another flower.

Each special-mother-cell contains within its cuticularized wall a mass of protoplasmic contents which have assumed a frothy condition owing to the presence of a number of vacuoles or oildrops. In this protoplasm spherical granules are to be met

[1] *Lessons in Elementary Botany*, p. 216.

with in considerable numbers and a distinct nucleus may be detected.

Very soon however by the aid of reagents, especially the aniline color methyline-blue[1], a delicate thin transparent hyaline membrane or wall is found to clothe and to have been formed all over its surface by the protoplasm, which has in some cases where the preparation has been treated with alcohol slightly contracted away from this wall. The membrane is however exceedingly difficult to detect at this stage. This change takes place simultaneously in all the special-mother-cells. The newly formed cell, consisting of a very thin and delicate cellulose wall, closely applied to the internal side of the pale yellow cuticularized wall of the special-mother-cell by which it is surrounded, but from which it may be made to contract away by means of alcohol, enclosing protoplasm loaded with vacuoles and rendered dark with minute granules, and a nucleus, is the equivalent of the pollen-grain of other plants, and will in future to indicate this feature be designated by the same title. The mode of formation of the pollen in Asclepias is very different to that which is the characteristic and prevalent type in the majority of Dicotyledons or Monocotyledons; and, so far as our present knowledge extends, exhibits in its *entire* details a perfectly unique, isolated and peculiar case of development. The earlier stages are only to be found paralleled in the single instance of Zostera, which affords either the most primitive or most aberrant type of pollen-formation known. The later stages find no precise parallel in the entire range of the vegetable kingdom. This is the more remarkable since another member of the Asclepiadeæ, viz. Periploca graeca, exhibits according to Reichenbach, a type of pollen-formation exactly comparable to that of the Orchid genera Neottia and Epipactis[2].

Observations on the mode of development of the pollen in Asclepias are fraught with extreme difficulty, and its history can only be revealed by careful study of extremely thin transverse and longitudinal sections.

In many of the pollen-grains, especially when the flower was fully mature, I was able by careful observation and by having recourse to osmic acid of one per cent. strength and to staining reagents such e.g. as Haematoxylon, Grenicher's carmine, and

[1] I owe the suggestion that I should make use of this staining reagent to my friend Mr W. Gardiner, who has employed it largely in his researches on "The continuity of the protoplasm in the motile organs of leaves."

[2] Dr S. H. Vines suggests that probably in Asclepias and likewise in Zostera the phase of the special-mother-cells as it occurs in other plants is omitted, and hence we get the marked departure from the normal types. On this view what I have termed the special-mother-cells are really the last series of mother-cells produced by repeated division of the single primary one.

some of the aniline colours, viz. Gentian-violet, Saffranin and Methyl-green (to the latter of which a few drops of solution of Acetic Acid, one per cent. strength, had been previously added), to detect not a single nucleus only, but two nuclei one of which was invariably larger than the other.

The smaller nucleus was often found lying close to the cell-wall, and in these cases I believe that, surrounded by a small quantity of protoplasm, it is cut off from the rest of the grain by a cellulose-wall although I was not always able to shew this satisfactorily. This discovery is especially of interest in connection with the recent researches of Strasburger[1] and Elfving[2] since further confirmation of their observations has thereby been obtained in the pollen-grains of plants which they did not investigate, and in which the very presence of nuclei of any kind whatever had not been previously detected. I consider the smaller nucleus of the Asclepiad pollen-grain to be the representative of what Elfving terms the "vegetative nucleus" and others have dignified as the "passive nucleus," which nucleus is genetically the last remnant of the male prothallium of a Vascular Cryptogam type such as Equisetum; while the larger nucleus, equivalent to the "active nucleus", is genetically the last remnant of the antheridium of such a type.

In shape the pollen-grains are always nearly spherical, though usually slightly angular, so as to be really irregularly polyhedral; their membrane, is as previously stated, single, very thin at first, ultimately becoming thicker, smooth hyaline and transparent, and formed of unchanged cellulose[3]. There is at this stage no appearance whatever of the tubes which are afterwards produced.

Strasburger[4], in his most recently published work, mentions the fact that he has observed the presence of only a single coat in the pollen grains of the following plants—*Gaura biennis* L., *Clarkia elegans* Dougl., *Senecio vulgaris* L., *Cobœa scandens* Cav., *Allium* L. *Naias major*[5]?, and Orchids, and the same phenomenon was described by Fritzsche[6], and has long been known to occur in

1 "Ueber Befruchtung und Zelltheilung." *Jenaische Zeitschrift für Naturwissenschaft*, Bd. XI, Neue Folge, Bd. IV. 1877, Heft IV. page 450.

2 *Jenaische Zeitschrift für Naturwissenschaft*, 1879, part I., and *Quarterly Journal of Microscopical Science*, N. S. Vol. xx. 1880, pp. 19 to 35.

3 I have never seen it present the irregular internal thickenings which are frequent in the pollen-grains of some plants, e.g. Cucurbita, and which are there used subsequently in the formation of the pollen-tube.

4 *Ueber den Bau und das Wachsthum der Zellhäute*, Jena, 1882.

5 Ueber den Pollen.—*Mémoire présenté à l'Académie impériale des Sciences de St Pétersbourg*, iii. 1837.

6 Hofmeister (*Neue Beiträge*, 1859, part ii.) describes the existence of "a very thin but distinct extine" in the pollen-grains of this species, but in his figure, pl. i. fig. 11, he represents this extine as extending with the intine along the course of the pollen-tube produced from the grain! It is therefore highly probable that Strasburger's observation is more accurate.

Zostera L., while *Asclepias Cornuti* Dcne. must now be added to this interesting list of exceptions to what is the otherwise universal rule in Phanerogamous plants.

I may here allude to the circumstance that after extremely careful observations many times repeated I have at length in several cases satisfactorily traced the passage for some distance of the larger of the two nuclei in the pollen-grain into the long cellulose tube which it puts forth when the pollinium ruptures in consequence of its being placed in immediate contact with the stigma, although I have been unable to follow its changes further. The larger nucleus of the pollen-grain does not then in this genus become broken up and diffused through the protoplasmic contents of the grain immediately before the production of the pollen-tube, as Strasburger has shewn that it does in many other flowering plants, but simply passes in its entire concrete form into the pollen-tube. What becomes of the smaller nucleus I cannot definitely state, but I have reason to believe that it is left in the grain, or perhaps dissolved, since I have never been able to trace its passage into the pollen-tube, although I have carefully watched for it.

The ultimate changes and fate which the tapetal membrane undergoes appear to be as follows:—

The cells composing it which lie on the outer side of the anther divide, each by means of a vertical tangential wall, parallel to the original tangential walls of the cell, so that the membrane becomes two cells broad on this side. Those tangential cell-walls, which are farthest from the pollinium, in that row of limiting cells which is next the cavity of the loculus, together with the adjacent portions of the radial walls of these cells become broken down and disintegrated. On the other hand the tangential walls which are nearest the pollinium, together with the internal[1] portions of the said radial walls, persist for some time forming a continuous membrane surrounded by a layer of small cells. These latter are on the outer side of the anther, segments from those cells derived from the parenchyma that completed the tapetum *proper* on this aspect, and on the inner side of the anther are the row of cells formed immediately external to the tapetum *proper* at the same time that it was differentiated and which have persisted. Such is the condition immediately prior to the opening of the two loculi to expose the pollinia, the method of which is intimately connected with the presence of other contrivances in the flower for ensuring pollination by the agency of insects. This change occurs by almost the whole of the parenchymatous tissue forming the substance of the anther, together with the remains of the

[1] With relation only to the loculus.

tapetal membrane and the upper portion of the inner epidermis which is never cuticularized becoming broken down; an external wall of several layers, and also in the lower two-thirds of the loculus an internal, alone persisting, while in the upper third the pollinium is freely exposed on that side which is directed internally, a large oval longitudinal but slightly obliquely directed aperture in the anther-wall being formed opposite each. This disintegration appears to take place first at the apex of the loculus and gradually to proceed downwards. The apices of the pollinia are thus exposed first, and each pollinium immediately on its exposure becomes attached to the free end of one of the "appendages" of a corpusculum which directly impinges upon it, and the end of which is still in a semifluid highly viscid condition. The attachment takes place just below the apex of the pollinium, which is pear-shaped, the narrow end of the pear being directed upwards and slightly obliquely away from the median line of the anther, since the loculus in its superior portion comes actually to abut by one end upon the extreme external wall of the anther itself. It has the form of a minute adhesive surface formed by the flattening of the viscid end of the "appendage" against the external coat of the pollinium. The two parts though externally united are however never confounded. The pollinia being thus firmly held from above, the rest of the parenchymatous tissue disintegrates, and the pollinia are left hanging freely suspended in the two open pouches of the anther, and in no way adherent to any portion of its substance; the pair are separated only by the median vertical dissepiment of the anther which persists connecting the inner and outer sides of the anther between the two cavities.

Jacquin[1], who examined the anthers only in their adult condition when they had already dehisced in this introrse fashion, naturally regarded the anthers in which the pollinia lay freely immersed as "antheriferous sacs" and the pollinia themselves as the true anthers; in this he was followed by Kölreuter and many others, and when Schreber[2] in 1789 insisted that these sacs of Jacquin were really anthers, he was instantly denounced by an indignant host of authorities, although his opinion has been since most amply confirmed and borne out. The comparatively late period to which the tapetal membrane persists in Asclepias is a noteworthy point: in other Dicotyledons it usually breaks down in consequence of the growth of the pollen-grains after the absorption of the walls of their special-mother-cells; while in the group of the Monocotyledons it becomes either diffluent or ab-

[1] *Selectae Stirpes Americanae*, 1763, p. 82.
[2] *Genera Plantarum*, p. 166 et seq.

sorbed at an early period and the mother-cells themselves in consequence float freely about in the loculus quite separate from one another. Asclepias therefore appears to present at first sight in the period of the resolution of its tapetum a closer analogy to the Monocotyledons than to the group of which it is a member; since the pollinium, which consists among other parts of the persistent though altered walls of the mother-cells, comes ultimately to lie in the cavity formed by its resolution. Inasmuch however as the period of its resolution is coincident with that of the dehiscence of the anther-loculus I believe that it more closely approaches the type of its own group than that of the Monocotyledons, though it differs from both so far as we know of them at present. It is further an important feature that there exists in the anthers of Asclepias no provision which shall determine their dehiscence, such as takes place in other plants by the reticulate thickening of the walls in a layer of cells immediately internal to the epidermis and extending round a portion of the area of the loculus, i.e. the so-called "endothecium" of Purkinge[1]. Schleiden's statement (*loc. cit.*) that the portion which disappears is dry and elastic, and is cast off as a valve is obviously inaccurate on this point. The nearest instance which I have been able to find to the type which they exhibit is a case described by Hofmeister in which the anther-lobes open at the apex by a pore which results simply from the destruction of a small portion of tissue at this spot, but probably other instances also will not be wanting when we have more extended knowledge on this point, for as yet very little indeed has been done in determining exactly the various modes in which the dehiscence of the anther takes place in different plants.

(4) *On some micro-organisms and their relations to disease.* By G. F. Dowdeswell, M.A.

[1] *De Cellulis Antherarum fibrosis.*

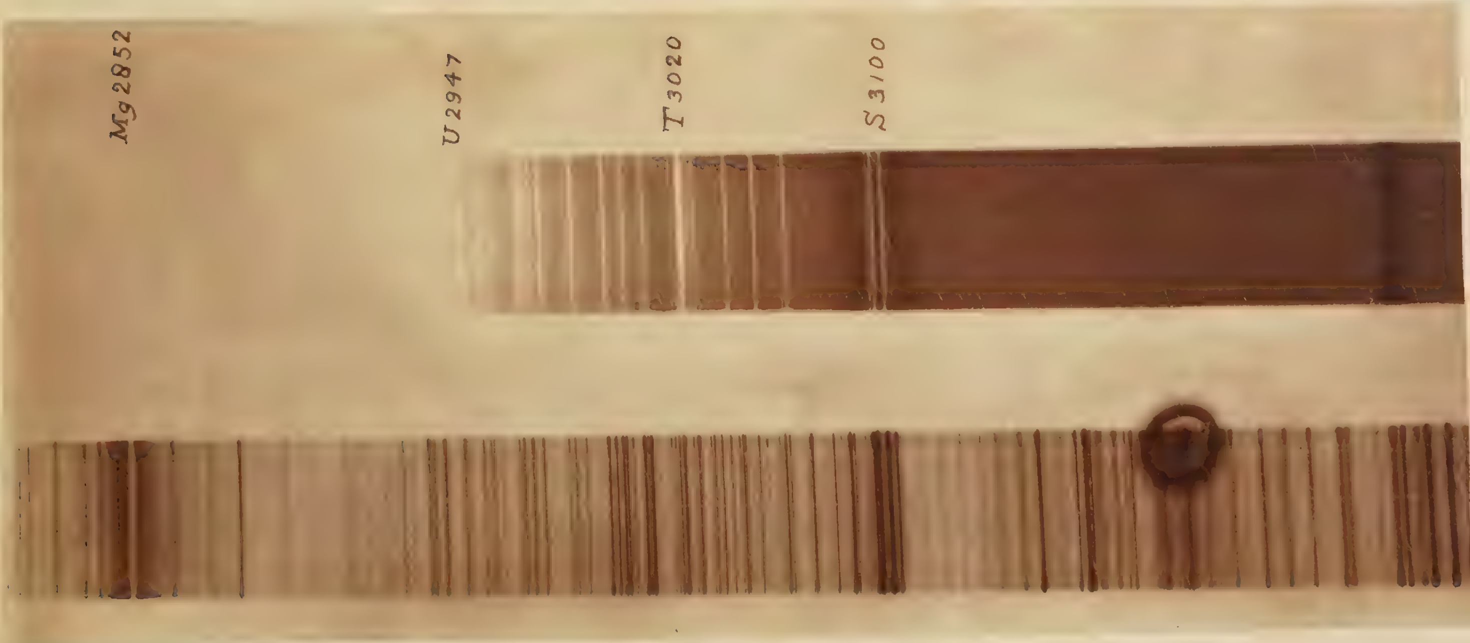


PLATE I. The lower photograph shews on the left self-reversal of *Mg* line w.l. 2852.
The upper shews the same line enormously expanded and the iron lines between *U* and *S* reversed against the bright wing of the expanded line.

PLATE II. The upper photograph shews the potassium lines reversed, one sodium line being also reversed, and, less strongly, two calcium triplets.

The lower photograph shews the spectrum of the arc before the potassium salt was introduced.

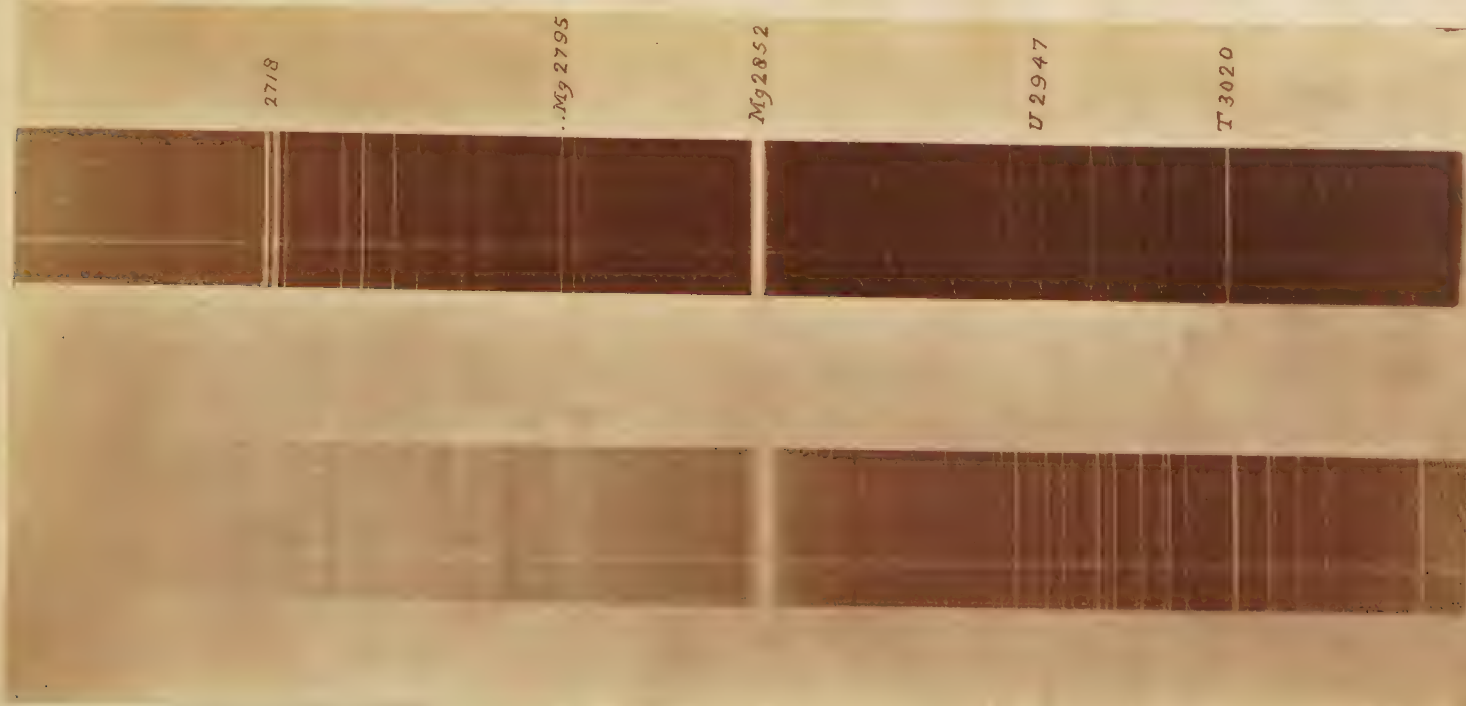
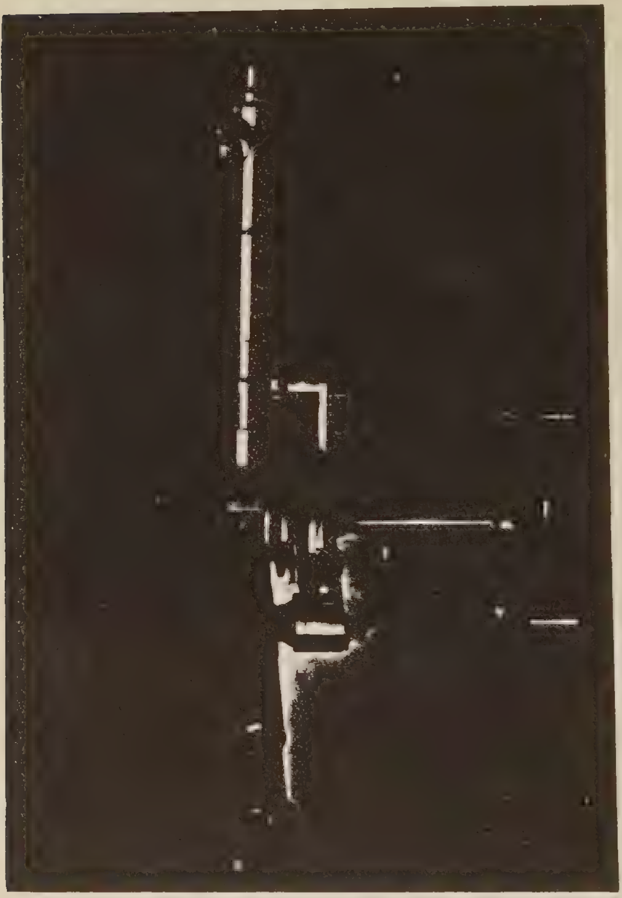


PLATE III. Shews iron lines reversed by introducing potassium ferrocyanide into the arc.

PLATE IV. Shews reversal of iron and other lines by a gentle stream of coal gas.

PLATE V. Shews iron and other lines reversed against the hot walls of a carbon tube. In the middle, where there was no background, the same lines are seen unreversed.



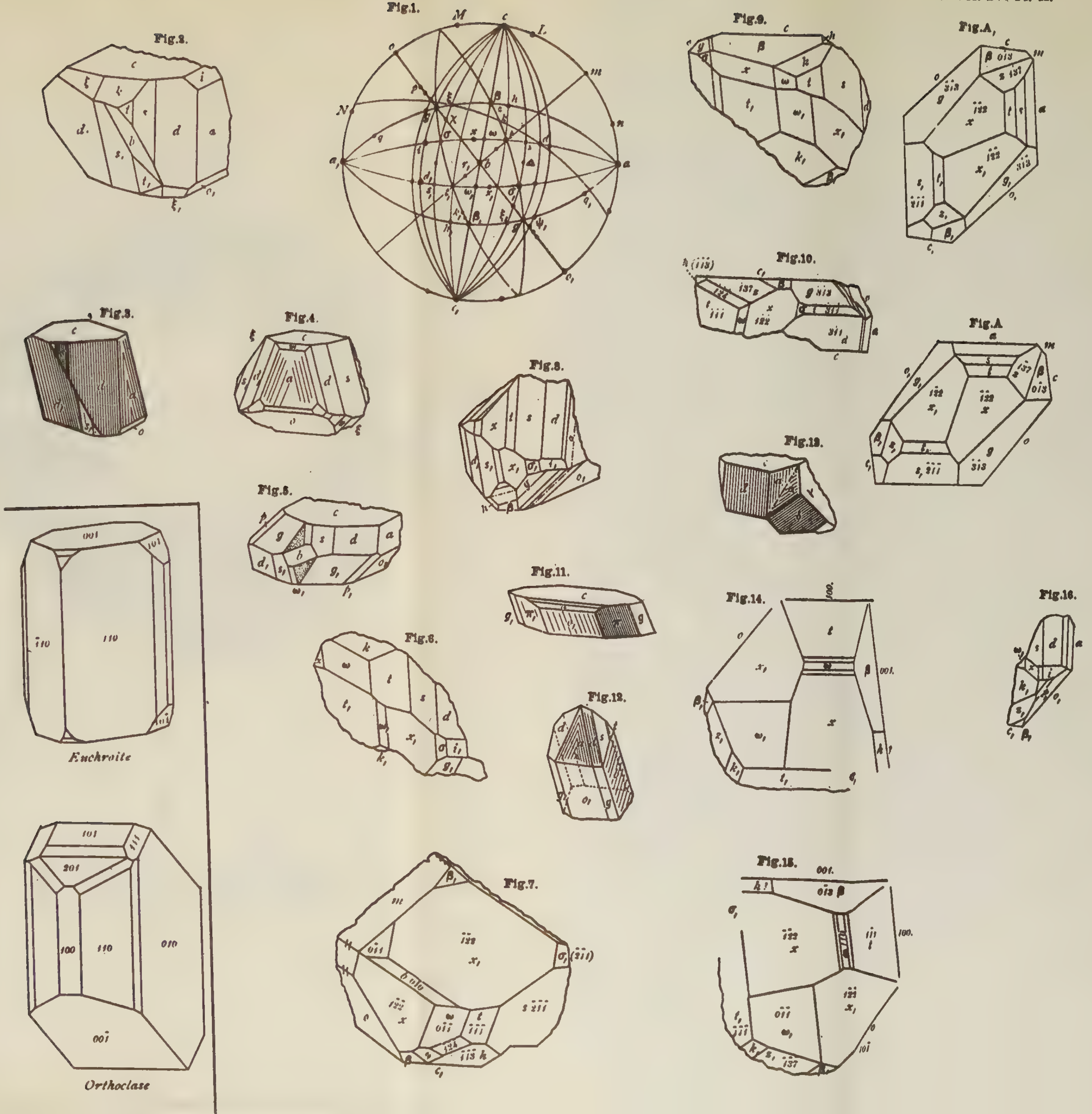


Fig.1.
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Fig.A
Euchroite
Orthoclase

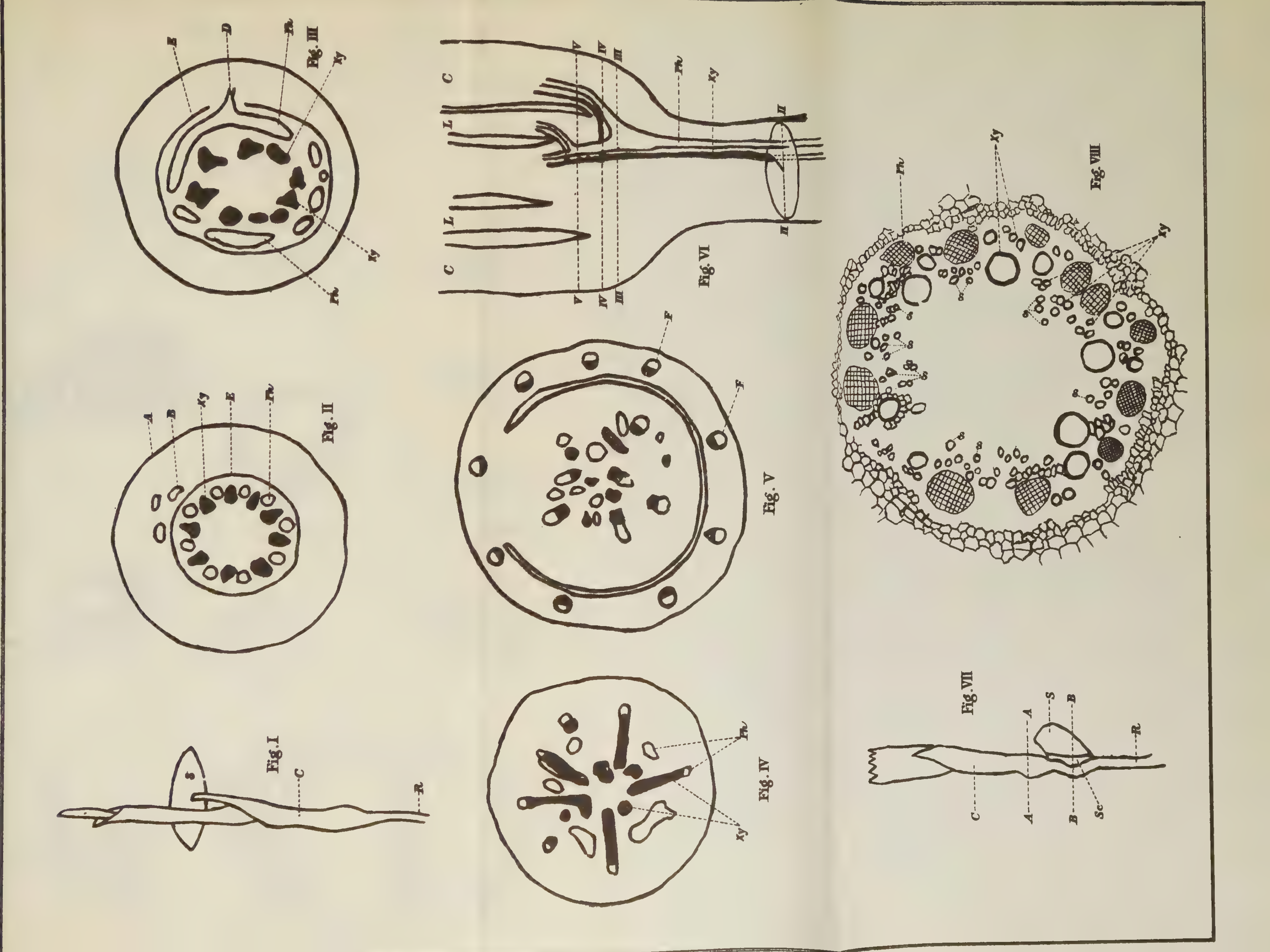
Fig. I
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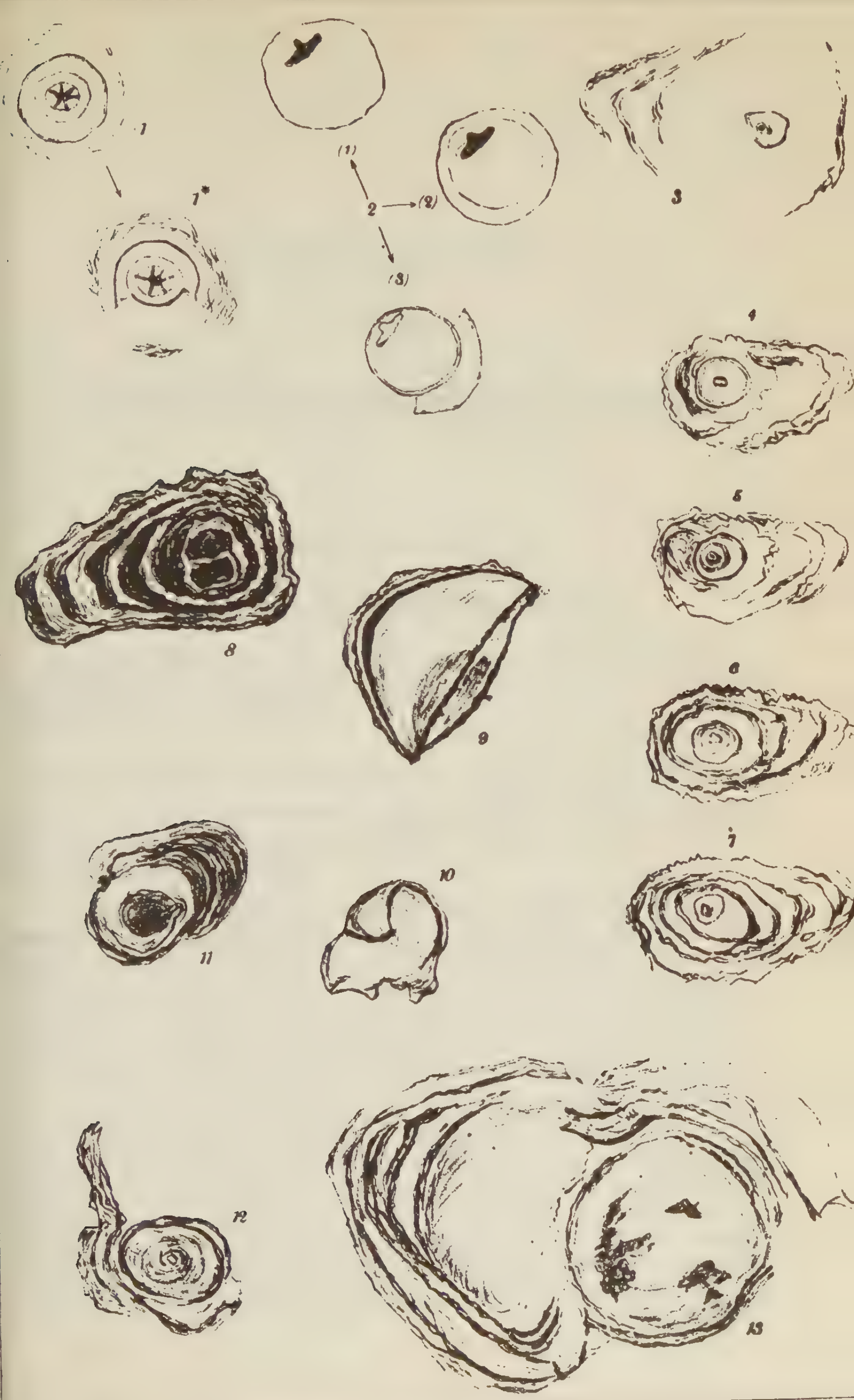

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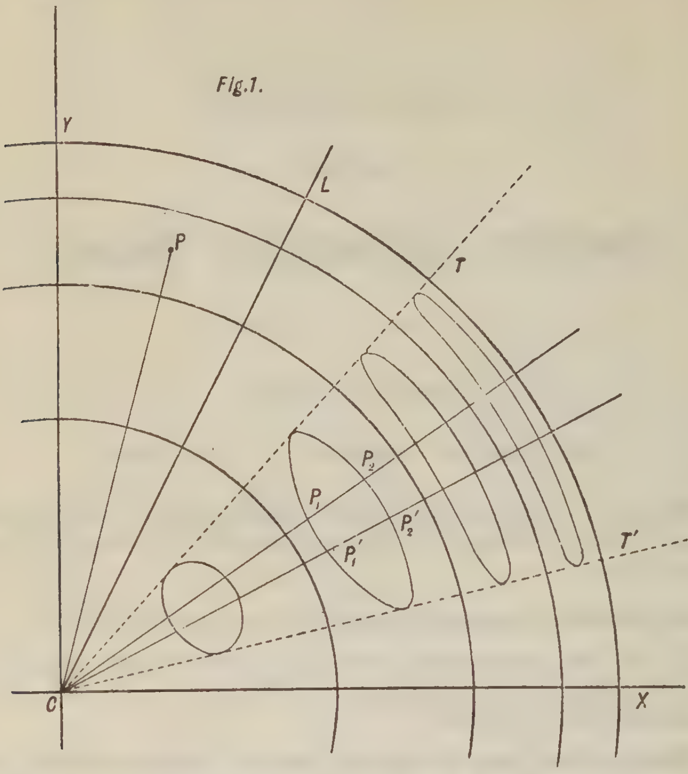
incident light, V the retardation produced by the plate at P, γ the angle between the planes of polarization and analyzation, and ρ

Fig. 1.



the amplitude of the incident light, then if I be the intensity of the light at P,

$$I = \rho^2 \left\{ \cos^2 \gamma - \sin 2\theta \sin 2(\theta - \gamma) \sin^2 \frac{\pi V}{\lambda} \right\}.$$

Again, if r denote the distance CP it is easy to shew that V is proportional to r^2, so that we may put $V = \frac{r^2}{a}$, a being constant. Thus if we put $a\lambda = c^2$ we have

$$I = \rho^2 \left\{ \cos^2 \gamma - \sin 2\theta \sin 2(\theta - \gamma) \sin^2 \pi \frac{r^2}{c^2} \right\}.$$

The curves of equal intensity then are given by $I = \text{constant} = \rho^2 \cos^2 \beta$ suppose, and by giving β all possible values from 0 to 90° we can trace the curves.

Clearly if $\beta = \gamma$ we have

$$\sin 2\theta \sin 2(\theta - \gamma) \sin^2 \frac{\pi r^2}{c^2} = 0.$$

Thus the four straight lines given by

$$\theta = 0, \ \theta = \gamma, \ \theta = \frac{\pi}{2}, \ \theta = \gamma + \frac{\pi}{2}$$

and the series of circles given by $r = c\sqrt{n}$ are included in the family. Along them the intensity is $\rho^2 \cos^2 \gamma$.

But the ordinary theory supposes that the intensity is constant along *any* circle with the origin as centre, and this is clearly wrong, for though, if r be a constant, $\sin^2 \pi \frac{r^2}{c^2}$ is constant, yet the term $\sin 2\theta \sin 2(\theta - \gamma)$ varies from point to point along this circle.

The curves of equal intensity are really given by

$$\sin 2\theta \sin 2(\theta - \gamma) \sin^2 \pi \frac{r^2}{c^2} = \text{a constant},$$

and we require to trace them.

The value of the constant is $\cos^2 \gamma - \cos^2 \beta$, $\rho^2 \cos^2 \beta$ being the intensity along the curve in question.

Thus

$$\sin 2\theta \sin 2 (\theta - \gamma) \sin^2 \frac{\pi r^2}{c^2} = \cos^2 \gamma - \cos^2 \beta.$$

If $\cos^2 \beta$ is $< \cos^2 \gamma$ so that the intensity considered is less than that of the brushes, $\sin 2\theta$ and $\sin 2(\theta - \gamma)$ must have the same sign.

This will be the case if θ lies between γ and $\frac{\pi}{2}$ or $\gamma + \frac{\pi}{2}$ and π or $\gamma + \pi$ and $\frac{3\pi}{2}$ or $\gamma + \frac{3\pi}{2}$ and 2π; while, if $\cos^2 \beta$ is $> \cos^2 \gamma$, so that the intensity is greater than that of the brushes, $\sin 2\theta$ and $\sin 2 (\theta - \gamma)$ are of opposite sign, and θ is between 0 and γ, $\frac{\pi}{2}$ and $\gamma + \frac{\pi}{2}$, or π and $\gamma + \pi$, or $\frac{3\pi}{2}$, and $\gamma + \frac{3\pi}{2}$.

Thus throughout the octants into which the field is divided by the brushes the intensity is alternately less and greater than that of the brushes.

Consider any line through the origin making an angle α suppose with the initial line. Then along this line

$$\sin\frac{\pi r^2}{c^2} = \pm\sqrt{\left\{\frac{\cos^2\gamma - \cos^2\beta}{\sin 2\alpha \sin 2(\alpha-\gamma)}\right\}}$$

Let ϵ be the least value of $\frac{\pi r^2}{c^2}$ which satisfies this equation.

Then $$\frac{\pi r^2}{c^2} = n\pi \pm \epsilon.$$

The circles of uniform intensity $\rho^2\cos^2\gamma$ are given by $\frac{\pi r^2}{c^2} = n\pi$. Thus between each two such consecutive circles a curve of given intensity $\rho^2\cos^2\beta$ cuts any radius vector twice in the points given

by $$\frac{\pi r^2}{c^2} = n\pi + \epsilon,$$

and $$\frac{\pi r^2}{c^2} = (n+1)\pi - \epsilon.$$

If however $\epsilon = \frac{\pi}{2}$ so that α is such that

$$\sin 2\alpha \sin 2(\alpha-\gamma) = \cos^2\gamma - \cos^2\beta,$$

the two points coincide.

Each curve of constant intensity $\rho^2\cos^2\beta$ consists therefore of a series of closed loops lying between the consecutive circles of the series

$$\frac{\pi r^2}{c^2} = n\pi.$$

This had been noticed by Mr W. D. Niven in a paper treating of the same subject in the *Quarterly Journal of Mathematics*, Vol. XIII.

Moreover the straight lines given by

$$\sin 2\theta \sin 2(\theta-\gamma) = \cos^2\gamma - \cos^2\beta$$

meet each of the loops which make up the curve of intensity $\rho^2\cos^2\beta$ in two coincident points.

Again, taking logarithmic differentials of the equation we find

$$\left\{\cot 2\theta + \cot 2(\theta-\gamma)\right\}\frac{d\theta}{dr} + \frac{2\pi r}{c^2}\cot\frac{\pi r^2}{c^2} = 0.$$

Hence, when $\frac{\pi r^2}{c^2} = (2n+1)\frac{\pi}{2}$ the radius vector and the tangent to the curve coincide.

Thus the lines CT, CT' in the figure given by

$$\sin 2\theta \sin 2(\theta-\gamma) = \cos^2\gamma - \cos^2\beta$$

touch all the curves of intensity $\rho^2 \cos^2\beta$, and the points of contact lie on the circles $r = c\sqrt{\left(\frac{2n+1}{2}\right)}$.

Again, along any radius vector when $\sin^2 \frac{\pi r^2}{c^2} = 1$ the intensity is a minimum if $\sin 2\theta \sin 2(\theta-\gamma)$ is positive, and a maximum if this expression is negative.

Thus the circles given by $r = c\sqrt{\left(\frac{2n+1}{2}\right)}$ are curves of minimum or maximum intensity.

The points of minimum and maximum intensity are the points of intersection of these circles and the lines given by

$$\cot 2\theta + \cot 2(\theta-\gamma) = 0,$$

that is by $\sin 2(2\theta-\gamma) = 0$, or by $2(2\theta-\gamma) = n\pi$.

Again, let P_1P_2 be the points in which a radius vector cuts the same loop of a curve of intensity $\rho^2\cos^2\beta$. Let $CP_1'P_2'$ be an adjacent radius vector, and, C being the origin, let the angle

$$P_1CP_1' = \delta\theta.$$

If, as before, ϵ is the least angle whose sine is equal to

$$\sqrt{\left\{\frac{\cos^2\gamma - \cos^2\beta}{\sin 2\theta \sin 2(\theta-\gamma)}\right\}},$$

then
$$CP_1^2 = nc^2 + \frac{\epsilon}{\pi}c^2,$$

$$CP_2^2 = (n+1)c^2 - \frac{\epsilon}{\pi}c^2,$$

and the area $P_1P_2P_2'P_1' = \frac{1}{2}(CP_2^2 - CP_1^2)\,\delta\theta$

$$= \frac{c^2}{2}\left(1 - \frac{2\epsilon}{\pi}\right)\delta\theta.$$

Since this is independent of n the element of each of the loops intercepted between these two straight lines is of the same area. Thus all the loops which together make up the curve of intensity $\rho^2\cos^2\beta$ are of the same area.

Thus instead of being circles the curves of constant intensity consist of a series of isolated ovals, with these two properties:—

(1) All the ovals of the same intensity touch two of four certain straight lines in points given by

$$r = c\sqrt{\left(\frac{2n+1}{2}\right)}.$$

(2) All the ovals of the same intensity are equal in area. There are moreover a series of points of maximum and minimum intensity determined by the intersection of the circles

$$r = c\sqrt{\left(\frac{2n+1}{2}\right)}$$

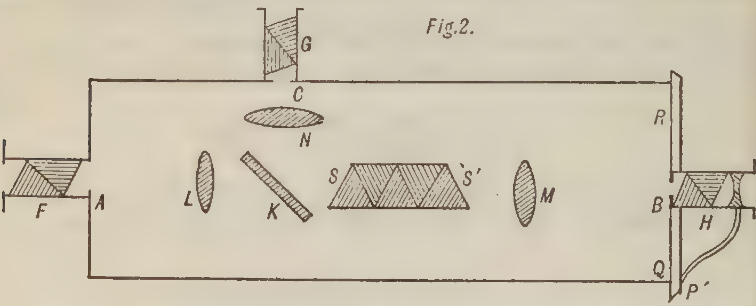
and the lines

$$2\theta = \gamma + \frac{n\pi}{2}.$$

(2) *On a Spectrophotometer.* By R. T. GLAZEBROOK, M.A., F.R.S.

The instrument was designed to determine the amount of absorption of coloured solutions for the various rays of the spectrum or to estimate by comparison with a standard solution the amount of colouring matter in any given absorbing solution. It consists of a long flat rectangular box (Fig. 2);

Fig. 2.



at one end of this there is a slit A, the width of which can be adjusted. The white light from a source behind the slit passes through a collimating lens L placed at the distance of its own focal length from A and falls as a parallel pencil on the set of direct vision prisms SS'. The emergent beam is brought to a focus by the second lens M and a pure spectrum formed on the end of the box.

A sliding piece fitted to this end carries a narrow slit B, and through it any desired part of the spectrum may be viewed, C is a second slit illuminated also by white light, the rays from which after passing through the lens N fall on a plane mirror K, and

being there reflected traverse the prisms and form a second spectrum directly below the first. By adjusting the positions of the lenses and the mirror K the lines in the two spectra can be made to coincide. The light from A passes over the top of the mirror and the two spectra are seen one above the other. A concave lens enables the observer to focus distinctly the line of separation. In front of each of the slits respectively are three Nicol's prisms F, G, H.

F is fixed with its principal plane vertical, parallel therefore to the slits and edges of the prisms, G has its principal plane horizontal, while H is capable of rotation round a horizontal axis parallel to the length of the box, P is a pointer fixed to the prism H and moving over a graduated circle QR which is divided into 360 parts. The zero of the graduations is at the top of the circle and when the pointer reads zero the principal plane of H is vertical.

The Nicols F and G polarize the light coming through the slits, the first in the horizontal plane, the second in the vertical. The emergent beam is analysed by the Nicol H. When the pointer reads zero or 180° all the light in the upper spectrum from the slit A can pass through H, none of that from C is transmitted. As the Nicol is rotated through 90° the quantity of light from A which is transmitted decreases, while the amount coming from C increases, and when the Nicol has been turned through 90° all the light from C is transmitted and none from A.

For some position then between 0 and 90° the brightness in the small portions of the two spectra viewed will be the same. Let the reading of the pointer when this is the case be θ. Let the amplitude of the disturbance from A be a, that of the disturbance from C be c, then clearly

$$a \cos \theta = c \sin \theta,$$

and if I_a, I_c are the respective intensities

$$\frac{I_a}{I_c} = \frac{a^2}{c^2} = \tan^2 \theta.$$

Now place anywhere between L and K a small rectangular cell containing an absorbing solution. The upper spectrum will become darker and the Nicol require to be moved to establish equality again in the brightness. Let θ' be the new reading and I'_a the intensity of the light which now reaches the eye from A. Then

$$\frac{I'_a}{I} = \tan^2 \theta'.$$

Thus

$$\frac{I'_a}{I_a} = \frac{\tan^2 \theta'}{\tan^2 \theta}.$$

But if k represent the proportion of light lost by absorption and reflexion at the faces of the vessel, we have

$$I'_a = I_a (1 - k).$$

Hence
$$k = 1 - \frac{\tan^2 \theta'}{\tan^2 \theta}.$$

To eliminate the effects of the vessel the experiment should be repeated with the vessel filled with water or some other fluid in which the absorption is small; the difference between the two results will be the absorption due to the thickness used of the absorbing medium.

Of course in all cases four positions of the Nicol can be found in which the two spectra will appear to have the same intensity. At least two of these positions—which are not at opposite ends of the same diameter—should be observed and the mean taken. In this manner the index error of the pointer or circle will be eliminated. A number of observations made at my request by Mr F. W. Sanderson, B.A. of Christ's College, seem to shew that the greatest difference between any two of a set of 6 or 8 readings will be from 2° to 3°, while the mean error of twelve or fourteen such sets is considerably under 1°; this result is confirmed by my own observations.

The following Table gives a series of values of θ, θ', and k obtained by Mr Sanderson when the absorbing medium was a small glass cell of about 1 c.m. thick filled with water, to which a slight blue tinge had been given by the addition of a few drops of a solution of sulphate of copper.

Colour.	θ.	θ'.	k.
Red, near C	60° 50′	49° 50′	·563
Green, near F	61° 30′	56° 30′	·338
Blue green	64° 30′	58° 30′	·352

To consider the question a little more fully, let us, following Bunsen and Roscoe, call the reciprocal of the thickness of the medium which allows one-tenth of the light to pass, the extinction coefficient. Let I' be the intensity of the light after passing a thickness m of the medium, I being that of the incident light, and E the extinction coefficient, then (Hüfner, *Quantitative Spectral-Analyse*) we have

$$mE = \log\left(\frac{I}{I'}\right),$$

and if C be the concentration of the solution, we can shew that $C = AE$, A being a constant for the medium.

Hence $$C = \frac{A}{m} \log \left(\frac{I}{I'}\right).$$

Thus $$C = \frac{A}{m} \log \left(\frac{1}{1-k}\right).$$

If now we are working with a solution contained in a glass vessel we must remember that much of the loss of light is due to reflexion at the faces of the containing vessel. To eliminate this strictly we should require to know the refractive indices of the glass and solution for the light in question, and then determine by means of Fresnel's formulæ the loss due to reflexion. The loss due to absorption will be found by subtracting this from the total loss, and it is the value of k thus obtained which we must use in the above formula. In practice this will be inconvenient, and a result which, when the absorption is considerable, will be near the truth may be obtained as follows.

Take a similar glass cell and fill it with water or some other medium which absorbs the light but little, while its index of refraction does not differ greatly from the solution whose absorption coefficient or concentration is required, place it in the path of the light, and observe the intensity. Let it be I_1 and let k_1 be the corresponding value of k,

then $$I_1 = I(1-k_1).$$

Now insert the absorbing solution and observe again. Suppose we find $$I_2 = I(1-k_2),$$

k_2 is the loss per unit intensity due to absorption and reflexion combined.

Since the refractive indices of the two media used are nearly the same the amount of loss due to reflexion will be the same in the two cases. Thus, if k denote the loss per unit intensity due to absorption only, $k = k_2 - k_1$ approximately.

Hence $$k = \frac{I_1 - I_2}{I}$$

$$= \frac{\tan^2\theta_1 - \tan^2\theta_2}{\tan^2\theta},$$

and we get finally

$$C = \frac{A}{m} \log \left(\frac{\tan^2\theta}{\tan^2\theta - \tan^2\theta_1 + \tan^2\theta_2}\right),$$

if we neglect the loss due to reflexion $\theta_1 = \theta$.

Thus to use the instrument to measure the amount of concentration of a solution, we must observe θ the position of the Nicol

when the two sources are viewed directly, θ_1 its position when the cell filled with water is in the way, and θ_2 its position when the cell filled with the liquid to be examined has been substituted for the water cell. The following table gives a series of observations taken in this manner by Mr Sanderson. Two strengths of solution were used, the values of θ_2 are indicated by θ_2 and θ'_2.

Colour.	θ.	θ_1.	θ_2.	θ'_2.
Blue	53° 30′	49° 30′	46°	44° 10′
Green...........	61° 30′	58°	54°	52° 10′
Red	56°	55°	48° 30′	39° 10′

Substituting in the formula above we find for the ratio of the strengths of the two solutions determined from the observations in the blue, green, and red respectively the values 1·46, 1·43 and 1·40 respectively. The values of θ, &c. in the above table for the green and red were the mean of two series which agree fairly closely, more uncertainty attaches to the values in the blue. The mean of the errors in the value of θ, &c. as determined from 5 or 6 observations is about 45′. These observations would seem to shew that the instrument is capable of comparing the strengths of two solutions to within 4 or 5 per cent. without difficulty.

A somewhat similar piece of apparatus has been described by Hüfner (*Journal für praktische Chemie*, Band XVI.), but in this case the light from only one source is plane polarized—it would in fact correspond to my apparatus without the Nicol G. Now by the reflexion at the mirror and refraction through the prisms this unpolarized light becomes partially polarized as the analysing Nicol is turned, therefore its intensity is altered, and an error of unknown—though calculable—amount is introduced into the observations. Arrangements of apparatus of the same nature have been used by Crova (*Comptes Rendus*, 1881), and Vierordt, *Die Anwendung des Spectral-Apparates zur Photometrie der Absorptionspectren.*

(3) *On a common defect of lenses.* By R. T. GLAZEBROOK, M.A. F.R.S.

The author exhibited some lenses which when placed between two crossed Nicol's prisms shewed strong elliptic polarization*.

* See letter by the author in *Nature*, Dec. 28, 1882.

(4) *On the motion of a mass of liquid under its own attraction, when the initial form is an ellipsoid.* By W. M. HICKS, M.A.

In three communications to this Society Mr Greenhill has fully discussed the problem of the motion of a mass of liquid which moves in space so as to keep its external form unaltered. His method differs from that of Dirichlet, who was the first to consider the question, in that he uses the Eulerian method in preference to the Lagrangian employed by Dirichlet, Dedekind and Riemann, and thus makes the theory simpler and clearer. I propose in the present communication to shew that the same method is applicable, with even greater simplicity, to the case where the mass of fluid has no rotation, and is initially in the form of an ellipsoid. It is known from Dirichlet's investigation that it will always keep the ellipsoidal form, the axes continually altering their lengths, yet so as to keep the volume constant.

The velocity potential for the motion of the fluid inside an ellipsoidal shell which changes its axes, without altering its volume, has been given by Bjerknes. If $\dot{a}$, $\dot{b}$, $\dot{c}$ are the rates of change of the axes a, b, c then

$$\phi = \tfrac{1}{2}\left(\frac{\dot{a}}{a}x^2 + \frac{\dot{b}}{b}y^2 + \frac{\dot{c}}{c}z^2\right)$$

with

$$\frac{\dot{a}}{a} + \frac{\dot{b}}{b} + \frac{\dot{c}}{c} = 0 \qquad \text{.........................(1).}$$

This is easily verified. The gravitation potential for points within the ellipsoid may be written

$$V = -k\left\{\Omega + \frac{x^2}{a}\frac{\delta\Omega}{\delta a} + \frac{y^2}{b}\frac{\delta\Omega}{\delta b} + \frac{z^2}{c}\frac{\delta\Omega}{\delta c}\right\},$$

where

$$\Omega = \int_0^\infty \frac{d\lambda}{\sqrt{\{(a^2+\lambda)(b^2+\lambda)(c^2+\lambda)\}}},$$

and k is constant when the volume is constant, as here. If we now suppose the fluid enclosed in an ellipsoidal shell which changes its form, the pressure at any point will be given by

$$\frac{p}{\rho} = \frac{\Pi}{\rho} - \tfrac{1}{2}\left\{\left(\frac{\ddot{a}}{a} - \frac{\dot{a}^2}{a^2}\right)x^2 + \dots\right\} - \tfrac{1}{2}\left(\frac{\dot{a}^2 x^2}{a^2} + \dots\right) - V$$

$$= \frac{\Pi}{\rho} - \left(\tfrac{1}{2}\frac{\ddot{a}}{a} - \frac{1}{a}\frac{\delta\Omega}{\delta a}\right)x^2 - \left(\tfrac{1}{2}\frac{\ddot{b}}{b} - \frac{1}{b}\frac{\delta\Omega}{\delta b}\right)y^2 - \left(\tfrac{1}{2}\frac{\ddot{c}}{c} - \frac{1}{c}\frac{\delta\Omega}{\delta c}\right)z^2.$$

If now the shell changes its axes so that

$$a\left(\tfrac{1}{2}\ddot{a} - k\frac{\delta\Omega}{\delta a}\right) = b\left(\tfrac{1}{2}\ddot{b} - k\frac{\delta\Omega}{\delta b}\right) = c\left(\tfrac{1}{2}\ddot{c} - k\frac{\delta\Omega}{\delta c}\right) \text{......(2)},$$

the pressure along the inside of the shell is uniform; in other words a uniform pressure over the surface will make the axes change according to the laws given by this equation. We may therefore suppose the shell removed, and the fluid will move by itself so that it always preserves the ellipsoidal form, and the magnitude of the axes at any time will be determined by equations (1) and (2).

A first integral is easily obtained, for each member of (2) is equal to

$$\frac{\tfrac{1}{2}(\dot{a}\ddot{a} + \dot{b}\ddot{b} + \dot{c}\ddot{c}) - k\left(\frac{\delta\Omega}{\delta a}\frac{da}{dt} + \frac{\delta\Omega}{\delta b}\frac{db}{dt} + \frac{\delta\Omega}{\delta c}\frac{dc}{dt}\right)}{\frac{\dot{a}}{a} + \frac{\dot{b}}{b} + \frac{\dot{c}}{c}},$$

and since the denominator vanishes by (1) and Ω contains t only as entering through a, b, c it follows that

$$\tfrac{1}{4}\frac{d}{dt}(\dot{a}^2 + \dot{b}^2 + \dot{c}^2) - k\frac{d\Omega}{dt} = 0,$$

whence $$\dot{a}^2 + \dot{b}^2 + \dot{c}^2 = 4k(\Omega - \Omega_0) \text{ ....(3)},$$

Ω_0 being a constant. This is the equation of the conservation of energy, and might have been thus deduced. Two particular cases are at once completely solved by means of (1) and (3), viz. the case of two axes always equal, or spheroids; and the case where one is infinite, in which case we get an elliptic cylinder. In the first case we have immediately, if $b = c$,

$$\frac{\dot{a}^2}{a^2} = \frac{4\dot{b}^2}{b^2} = \frac{8k}{2a^2 + b^2}(\Omega - \Omega_0),$$

and $ab^2 = r^3$, if r is the radius of the sphere whose volume is the same as that of the liquid. Putting in this value we have

$$\dot{a}^2 = \frac{8ka^3}{2a^3 + r^3}(\Omega - \Omega_0) \text{ ....(4)}.$$

The relation between the greatest and least values of a is found from the equation

$$\Omega = \Omega_0.$$

If we write $xa = r$ $(x < 1)$ for the greatest, and $a = yr$ $(y < 1)$ for the least, this equation becomes

$$r\Omega_0 = \frac{x}{\sqrt{(1-x^3)}} \log_e \frac{1+\sqrt{(1-x^3)}}{1-\sqrt{(1-x^3)}}$$

$$= 2\sqrt{\left(\frac{y}{1-y^3}\right)} \tan^{-1} \sqrt{\left(\frac{1-y^3}{y^3}\right)} \text{.........(5)}.$$

It will be found convenient in what follows to replace x, y by u, v where $1-x^3 = u$, $1-y^3 = v$; when the form never departs very largely from the spherical form u, v are not large, in this case we have very approximately

$$\left.\begin{aligned} r\Omega &= 2 - \frac{2}{45}\left(u^2 + \frac{64}{63}u^3 + \frac{58}{63}u^4\right) \\ &= 2 - \frac{2}{45}\left(v^2 + \frac{62}{63}v^3 + \frac{55}{63}v^4\right) \end{aligned}\right\} \text{..............(6)},$$

whence neglecting cubes and higher powers of u, v

$$\left.\begin{aligned} u_0 &= v_0 - \frac{1}{63}v_0^2 \\ v_0 &= u_0 + \frac{1}{63}u_0^2 \end{aligned}\right\} \text{.......................(7)}.$$

Equation (5) can be put into a very elegant form, easily adapted for the use of logarithmic tables, which give the logarithms of secants and tangents, by introducing angles θ, ϕ where

$$x = \sin^{\frac{2}{3}}\theta,\ y = \cos^{\frac{2}{3}}\phi;$$

when it will be found that (5) takes the form

$$\phi \cos^{\frac{1}{3}}\phi \operatorname{cosec} \phi = \sin^{\frac{2}{3}}\theta \sec\theta \log_e \cot \tfrac{1}{2}\theta$$

$$= 2{\cdot}3025851 \sin^{\frac{2}{3}}\theta \sec\theta \log \cot \tfrac{1}{2}\theta.$$

Corresponding sets of values of θ, ϕ are obtained with extreme ease by means of this equation, by giving a series of values to θ, finding the value of the logarithm of the right-hand member, and taking as a rough approximation

$$\phi = \frac{\pi}{2} - \theta.$$

A closer approximation can then be obtained by proportional parts.

From (4) we have

$$t=\frac{1}{2\sqrt{(2k)}}\int\sqrt{\left\{\frac{2a^3+r^3}{a^3(\Omega-\Omega_0)}\right\}}\,da,$$

where it must be remembered that Ω has different forms for the cases of $a>r$ and $a<r$. We consider these separately and determine the time from the spherical state to the greatest elongation or depression. If g denote the acceleration due to gravitation on the surface of the liquid when it has the spherical form it can easily be shewn that $4k=3gr^2$.

Putting in this value of k, it can be shewn, after some numerical calculations, that if t_1, t_2 denote the times from greatest elongation to the spherical form, and from the spherical form to the greatest compression respectively, then to the second order of u, v

$$t_1=\tfrac{1}{2}\sqrt{\frac{5r}{g}}\left\{\frac{\pi}{2}+\frac{19}{126}u_0+\left(\frac{1013}{42336}\pi+\frac{304}{3969}\right)u_0^2\right\}$$

$$t_2=\tfrac{1}{2}\sqrt{\frac{5r}{g}}\left\{\frac{\pi}{2}-\frac{19}{126}v_0+\left(\frac{1013}{42336}\pi-\frac{589}{2\times 3969}\right)v_0^2\right\},$$

whence if T denote the time of semi-oscillation, we have, from (7),

$$T=\frac{\pi}{2}\sqrt{\frac{5r}{g}}\left\{1+\frac{1013}{21168}u_0^2\right\};$$

the first term of this agrees with the result obtained by Sir W. Thomson* for the time of oscillation of a liquid sphere, slightly deformed according to a zonal harmonic of order 2.

Similarly in the case of the elliptic cylinder, it follows that

$$\dot{a}^2=\frac{4ka^4}{a^4+r^4}(\Omega-\Omega_0),$$

where $$2k=gr,$$

and $$\Omega=\int_0^\infty\frac{a\lambda}{\sqrt{\{(a^2+\lambda)(b^2+\lambda)\}}};$$

Ω is here infinite but $\Omega-\Omega_0$ is finite, viz.

$$2\log\frac{a_0+b_0}{a+b}.$$

Hence $$\dot{a}^2=\frac{4gra^4}{a^4+r^4}\log\frac{a_0^2+r^2}{a^2+r^2}\frac{a}{a_0}.$$

* "Oscillations of a liquid sphere." *Phil. Trans. Roy. Soc.* (1863), p. 608.

(5) *On functions of more than two variables analogous to Tesseral Harmonics.* By M. J. M. HILL, M.A.

[*Abstract.*]

A transformation analogous to the ordinary transformation from rectangular to polar variables is applied to an equation similar to that of Laplace, but containing i variables.

A solution of the transformed equation is found which consists of products of terms, each term being a function of one only of the new variables.

The solution thus found, omitting the first term, is called a Normal Function, and is analogous to a Tesseral Harmonic.

The Normal Functions possess a property analogous to the conjugate property of Tesseral Harmonics.

The value of an integral corresponding to the integral of the square of a Tesseral Harmonic over the surface of the sphere is next evaluated.

Then it is shewn that the solution of the transformed equation is a rational integral homogeneous function of the original variables, and that it includes under its form all the independent rational integral homogeneous functions of the original variables of the same degree which satisfy the original equation, but nothing else.

The expansion of an arbitrary function of $i-1$ variables in a series of Normal Functions concludes the paper.

(6) *Observations of the Transit of Venus across the Sun, taken near Kingston, Jamaica, at Cherry Garden, the residence of Oscar Marescaux, Esq.* Dec. 6, 1882: by Dr Pearson.

The Cambridge Philosophical Society will probably be glad to receive an early account of the observations of the Transit taken by one of its members. Their value will not be affected, though the interest taken in them may perhaps be increased, by the fact that they were made by one who belongs to the same college in this University as the sole spectator of the first transit ever observed, viz. that of Nov. 24 (o. s.), 1639.

The Latitude of my place of observation was determined, though from the nature of my instruments, not with very great precision, by meridian altitudes of the Sun, Fomalhaut and Achernar, the Pole-star not being available: and verified, in a way, by my known distance from the Lighthouse to the east of Port Royal, on the Palisadoes, at a place named Plumb Point: which bore only a degree and a half east of South, at a distance

of about seven miles. My observations vary between 18° 2½′ and 18° 3½′ North Lat. I have taken 18° 3′ 20″ as my basis of calculation, this being about the average given by the stars. Greater accuracy is clearly of no importance. The Longitude is derived from that of Rodney's statue in the town of Kingston, which was determined by Lieut. Green, of the U. S. Coast Survey, two or three years ago, by telegraphic signals made with extreme care, to be 5 h. 7 m. 10·71 s. W. of Greenwich. In my results I assume that I was about 1¾ s. east of this, or in 5 h. 7 m. 9 s. E. Long. of Greenwich: 5 h. 16 m. 30 s.* E. of Paris.

My Local Time I have been able to determine with considerable precision. From about Nov. 23rd to Dec. 2, and from Dec. 8 to 13, I was continually taking morning and afternoon altitudes of the Sun, as well as altitudes of the stars in the evening. From these I inferred the best chronometer of the two which I had with me,

		m.	s.			
to be	Nov. 25 a.m.	2	45½	slow on L. M. T.		
	Dec. 1 „	3	6	„	............D. L. R.	3·4 s.
	Dec. 13 „	3	48½	„	.... „	3·54 s.

Also, by the kindness of Dr Copeland and Capt. Mackinlay, from the clocks of the Government Transit Expedition at Up Park near Kingston, I obtained, Dec. 1st, Chronometer 3 m. 6 s. slow on their local Time, their meridian being within half a second of my own, and Dec. 13, 9 p.m. by Transits of γ^2 Ceti and σ Arietis, their clocks having been dismounted, 3 m. 49 s. which gives the chronometer a losing rate of 3·44 s. I have therefore assumed my chronometer to have been 3 m. 24 s. slow on L. M. T. at the morning contact; and 3 m. 24½ s. in the afternoon; which gives results without fractions of seconds, which situated as I was it would be needless to introduce.

The actual times of contact I had intended to take from my second chronometer, which has an extremely clear tick: unfortunately it stopped from the heat when it had been exposed five or ten minutes to the rays of the Sun, and before the time of the first contact; I was therefore compelled to use my watch which I held in my hand, though I do not think that any error

* Since my return I have found by the large Chart of Kingston Harbour, to be seen in the University Library, that the Lighthouse which I mention is as nearly as possible in 5 h. 7 m. 6 s. E. longitude reckoned by its position as referred to Rodney's Statue. But 7 m. × tan 1½° gives ·187 m. or nearly 1 s. of time (a sea-mile = 4 s.) as the difference between my own meridian and that of the Lighthouse. This would imply that my own W. longitude was 5 h. 7 m. 7 s. instead of 5 h. 7 m. 9 s. The position of the Government observatory at Up Park Camp appears from the chart to be nearly on the same meridian as my own: and this agrees with the result I obtained on the spot, by which I made the two agree within half a second. The L. M. T. therefore of my results must be increased in each case by two seconds. I shall look with interest to see what longitude Dr Copeland gives in his published observations.

can have resulted from my doing so, as I habitually observe in this way; the time on my watch I compared with my best chronometer directly after each contact.

The first external contact I unfortunately missed in a way which can be easily explained. Though constantly observing the Sun for altitude I have seldom examined its features; and thus failed to notice that a point on its disk, 145° E. of N. the predicted point of contact, would at 9 a.m. be slightly to the right and not to the left of the lowest point of the Sun's limb. Having for the three days previously been rather indisposed and unwilling to expose myself in observing, I had put my preparatory work somewhat aside; and thus failed to look through the question as I might perhaps have done, had I been quite ready for any kind of work.

When I saw Venus first she had intruded about one-third of her sphere on the Sun's disk; I watched her carefully until the two limbs were very nearly in contact, from which time I did not remove my eye until the Sun's light appeared to surround the planet; the moment of this phenomenon I fixed at

9 h. 16 m. 26 s. a.m. L. M. T.,

or perhaps two or three seconds later.

I noticed no kind of black drop, or sympathetic attraction, or assimilation between the limb of the planet and that of the Sun or rather the edge of the atmosphere enclosing the Sun. If the slight want of definition from which my vision suffered, be it due to my own eyes or my eye-piece or my object-glass, allows me to give any formulated description of the first internal contact, I should say that when the planet was actually projected on the Sun's disk, say 20 s. before the time I assign for actual contact, the black surface of the planet adjoining the atmosphere seemed to begin to be picked out with little white dots commencing very probably from either side; but as the phenomenon was new to me, I cannot say whether the white spots began at the two ends of the incomplete segment of the planet's disk, or whether they began throughout at once. I cannot say that I actually saw two horns of light gradually advancing until their points touched; but rather, as I have said, the segment of the planet nearest the atmosphere and still obscure began to be speckled with white dots which in not more than 20 seconds, or 25 at the outside, developed into a white line.

When the planet was a little way advanced on the Sun's disk, she was well defined in her outline: and no remarkable difference presented itself at 10·52 a.m. when I took a measure of the distance of her limb from the Sun's edge. But at noon, when the distance of the centres of the Sun and Venus was least, the irregularity of her disk, from the *boiling* of the surrounding solar

light, was very marked; and had increased at 1·35 p.m. when I think it was at its maximum.

I had little or no hesitation as to the time of the third, or second internal contact. I think the outer edge of Venus was too disturbed to exhibit the minute spots which I seemed to observe in the morning. But still the planet seemed to descend fairly upon the atmosphere without any mutual attraction of any kind, and though the planet's disk could not then have been called anything like a perfect curve or sphere, the actual contact seemed perfectly regular; and I do not think I had any hesitation as to when it actually occurred: *viz.* at

2 h. 41 m. 2 s. p.m. L. M. T.

I was not entirely so fortunate as I might have been in taking off the last external contact: owing to my mishap with my chronometer in the morning, I had to trust for the time to my watch, which I held in my hand: but just before the last contact through inadvertence I had allowed the planet to get too near the edge of the telescope to be viewed as it should be: so I took hold of the slow motion handles to bring her more into the field of view. Moving the telescope in this way causes a slight tremor in the telescope itself: and when this had passed off, the indentation caused by the planet's disk on that of the Sun had all but disappeared; and I could not feel perfectly confident within two or three seconds when the actual disappearance was complete. Still I am sure there can be no great error about the time I give, *viz.*:

3 h. 1 m. 15 s. p.m. L. M. T.

as the vanishing segment of the planet had only a partial resemblance to the undulations on the edge of the Sun; especially as the momentary apprehensions that I felt do not seem justified by the result. Had the period I assumed between the third and fourth contacts been too long, a mistake would be probable; but as it was eleven or seventeen seconds shorter than the computed time, and as I am sure that the Sun's disk had entirely recovered its normal outline at the time I noted, I think that my observation may be taken as generally exact. Moreover, the longer the period of transit, the shorter will be the passage across the Sun's limb: and as my own period of transit is as long as that computed by the French, and two minutes longer than that given by the Nautical Almanac, I may be satisfied with the fact that my passage across the disk is shorter than that anticipated by either of these authorities.

It has been a great matter of interest to me to see how nearly my own observations would agree with the times of ingress and egress announced in the Nautical Almanac and in the *Connaissance des Temps.* I have therefore computed them for my place of

observation according to the rules given, with the following results; for the sake of clearness and certainty, I give the actual computations for the first contact, as well as the results for the other three critical times.

The Nautical Almanac gives as the time of first contact for any place on the earth's surface, radius ρ, Geocentric North Latitude L, and East Longitude λ: Dec. 6th,

$$1 \text{ h. } 55 \text{ m. } 57 \text{ s.} + [2\cdot5471]\, \rho \sin L - [2\cdot4789]\, \rho \cos (\lambda - 87^\circ\ 53\cdot3').$$

The quantities in brackets being the logarithms of seconds of time.

If ϕ = Geographical, ϕ' = Geocentric latitude, $\phi - \phi' = c \sin 2\phi$. Supposing

$$c = \frac{1}{299\cdot15} \qquad l \sin 2\phi = \quad 9\cdot7703755$$

$$\begin{array}{rl} \log c = & 2\cdot4758890 \\ \hline & 7\cdot2944865 \\ & = \log\ \cdot0019701 \end{array}$$

$$\phi - \phi' \ldots\ldots\ldots\ldots\ldots\ldots = 6'\ 46''$$

$$\begin{array}{rl} \phi = & 18^\circ\ 3'\ 20'' \\ & \quad\ \ 6\ \ 46 \\ \hline \phi' = & 17\ 56\ 34 = L. \end{array}$$

$$\begin{array}{lrr} \rho = 1 - c \sin^2 \phi & = & 9\cdot4912763 \times 2 \\ \hline & & 8\cdot9825526 \\ & 1\cdot0000000 & 2\cdot4758890 \\ \hline \log & \cdot0003211 = & 6\cdot5066636 \\ \hline \rho = & 9\cdot9996789 & \end{array}$$

$$\begin{array}{rl} \lambda = & 360^\circ - 76^\circ\ 47'\ 15'' \\ = & 283^\circ\ 12'\ 45'' \\ \lambda - & 87^\circ\ 53'\ 18'' \\ = & 195^\circ\ 19'\ 21'' = \lambda' \end{array}$$

$$\begin{array}{rl} & 2\cdot54710 \\ \text{og } \rho = & 9\cdot99986 \\ \text{in } L = & 9\cdot48865 \\ \hline & 2\cdot03561 \\ & = 108\tfrac{1}{2} = 1'\ 48\tfrac{1}{2}'' \end{array} \qquad \begin{array}{rl} & 2\cdot47890 \\ \log \rho = & 9\cdot99986 \\ l \cos L = & 9\cdot97835 \\ l \cos (\lambda') = & 9\cdot98428 \text{ n.} \\ \hline & = 2\cdot44139 \text{ n.} \\ & = 4'\ 36\tfrac{1}{2}''\ (-) \end{array}$$

G.M.T. for first contact.

$$\begin{array}{rrrl} \text{h.} & \text{m.} & \text{s.} & \\ = 1 & 55 & 57 & \text{P. M.} \\ & 1 & 48\tfrac{1}{2} & \\ & 4 & 36\tfrac{1}{2} & \\ \hline 2 & 2 & 22 & \\ 5 & 7 & 9 & \text{W. Long.} \\ \hline 8 & 55 & 13 & \text{L. M. T.} \end{array}$$

The *Connaissance des Temps* while giving a similar formula for obtaining the equation of contact for any locality where the phase is visible gives a special formula for finding the Geocentric Latitude; which it says may be found by the relation $\tan \phi' = 0\cdot99666 \tan \phi$: as this will make $\phi' = 17^\circ\ 59'\ 57''$, I have ventured to read $9\cdot99666$ for $0\cdot9$ &c., implying a logarithm instead of a real number; which gives $\phi' = 17^\circ\ 55'\ 34''$.

The first contact will then be given approximately by

$$\text{P. M. T.} = 2 \text{ h. } 4 \text{ m. } 21 \text{ s.} + a_1 \sin \phi' - b_1 \cos \phi' \cos (\lambda + c_1) + \ldots\ldots \ \&\text{c.}$$

where $\log a_1 = 2{\cdot}54474$, $\log b_1 = 2{\cdot}48082$, $c_1 = 273^\circ\ 53'\ 4''$,

and consequently $(\lambda + c_1) = 194^\circ\ 46'\ 6''$.

Consequently:

$$\begin{array}{rlrl} & 2{\cdot}544740 & & 2{\cdot}480820 \\ & & L\cos\phi' = & 9{\cdot}978388 \\ L\sin\phi' = & 9{\cdot}488255 & L\cos(\lambda + c_1) = & 9{\cdot}985428\text{ n.} \\ \hline & 2{\cdot}032995 & & 2{\cdot}444636\text{ n.} \\ & = 108\text{ s.} = 1\text{ m. }48\text{ s.} & & = 278\tfrac{1}{2}\text{ s.} = 4\text{ m. }38\tfrac{1}{2}\text{ s.} \end{array}$$

And so we have

h. m. s.	
2 4 21	
1 48	
4 38½	
2 10 47½	
5 16 29½	W. Long.
8 54 18	L. M. T. as the moment of first contact.

As I have said at the beginning, the first contact was not observed by me, but I have no doubt that it occurred later than this, and probably later than the time deduced from the Nautical Almanac. The subjoined Table gives the times of the four contacts, as computed in London and at Paris, and as observed by myself*.

	London.	Paris.	Self.
	h. m. s.	h. m. s.	h. m. s.
First external contact...	8 55 13 a.m.	8 54 18 a.m.	
„ internal „	9 15 30 „	9 14 41 „	9 16 26 a.m.
Second „ „	2 37 52 p.m.	2 39 6 p.m.	2 41 2 p.m.
„ external „	2 58 19 „	2 59 28½ „	3 1 15 „

Again, for the intervals, probably more important than the actual times: a comparison gives

	London.	Paris.	Self.
	h. m. s.	h. m. s.	h. m. s.
Between 1st and 2nd contacts	20 17	20 23	
„ 2nd and 3rd „	5 22 22	5 24 25	5 24 36
„ 3rd and 4th „	20 27	20 21½	20 13

I also took five micrometrical measures of the distance between the limbs of the Sun and the planet: but my micrometer is new and not having ascertained the exact value of its divisions at present, I will defer giving my results in this communication. An approximate estimate of the diameter given by Venus when well advanced on the Sun's surface makes it to have been 63·4″, that given in the *Conn. des Temps* for that day being 62·8″.

* See note on page 314.

I may add that the object-glass of my telescope was made by M. Secretan, of Paris, and is of $3\frac{1}{2}$ inches diameter and 51 inches focal length. It is coated on the inner side with a silver film, which I found was quite sufficient to reduce the brightness of the Sun, when used, as I employed it, with a diagonal eye-piece. The mounting eye-pieces and micrometer were by Mr Simms; and I can take no exception to any part of the instrument. I unfortunately did not receive in time an eye-piece, power 120, constructed on the principles which I explained to the Society in October last; but I am not prepared to say that it would have removed the slight indistinctness which certainly I noticed with the power I actually used, viz. 135, for which I cannot entirely account, but which I am sure was practically not enough to affect the apparent time of contact, at any rate more than two or three seconds, within which similarly situated observers seldom agree. I saw nothing like an atmosphere round Venus, though I looked carefully for it: it is possible that my telescope, considerably smaller than what I may call the authorized size, would not be large enough to shew it. At the same time it is, as far as I can tell, tolerably achromatic: and I will take this opportunity of mentioning a phenomenon, possibly of a somewhat similar nature, which I have observed here more than once. The Sun could be seen from my place of observation to set behind a ridge of hills perhaps thirty miles distant. When examined with the telescope, at an elevation of about one degree above the horizon the lower limb was of an orange tint, the sides of the Sun's ordinary colour, and the upper limb green: these tints continuing as the two limbs successively disappeared behind the hills, which are often quite free from any cloud or haze. I do not recollect having seen the phenomenon described in any book: nor am I sure of its explanation.

In conclusion I should mention that nothing could be more favourable than the weather throughout the day. In the morning there was no sign of a cloud anywhere: and in the afternoon a few light fleecy clouds floating about did not intercept the Sun's rays at any time when I was actually observing.

February 12, 1883.

MR GLAISHER, PRESIDENT, IN THE CHAIR.

Mr T. H. Corry, B.A., Caius College, was balloted for and duly elected a Fellow of the Society.

The PRESIDENT referred to the terrible loss that Mathematics had sustained by the death of Professor H. J. S. Smith, of Oxford, an Honorary Member of the Society. In the Theory of Numbers

and its applications to Elliptic Functions he was without a rival, and this was also true of his knowledge of Modern Geometry. Besides his great report on the Theory of Numbers to the British Association, which was well known, he was the author of two memoirs of the highest importance on the subject, published in the Philosophical Transactions, of which it might probably be said with truth that nearly all the results on the subject that had since been obtained on the Continent were included in them as cases of the very general theorems they contain. The value of these papers could not be overestimated; the general principles enunciated in them gave by a uniform process all the theorems discovered by Jacobi, Eisenstein and Liouville relating to the representation of numbers by four squares and other simple quadratic forms. Besides these researches, Prof. Smith had given the complete solution of the problem of the representation of numbers by sums of squares. Eisenstein had shewn that the series of theorems relating to the representation by squares ceased when the number of them exceeded eight: the only cases that had not been considered were the extremely difficult ones of five squares and seven squares. These yielded to Prof. Smith's powerful analytical methods, and he gave the enunciation of the theorems for the case of five and seven squares in the Proceedings of the Royal Society for 1867. In ignorance that the problem had been solved fifteen years before, the question of the resolution into sums of five squares was proposed as the subject for the Mathematical Prize by the French Academy last year.

Much of the brilliant work performed by Prof. Smith in the younger years of his life remains unfortunately still unpublished. He looked forward to the termination of the labours of the Oxford University Commission that he might devote more time to the completion and publication of his mathematical work; and in the last year he revised more than 100 quarto pages of his great memoir on the Theta and Omega functions, which was to appear in the same volume as the Tables of the Theta Functions. In this memoir he gives the arithmetical theory of the transformation of Elliptic Functions and connects it with the transcendental theory of Jacobi. He also published during the year one 'note' and the half of a second one, occupying about 50 pages, in the *Messenger*, on Elliptic Functions when the modulus is complex: a subject of great difficulty and complexity and one well worthy of his powers. There were to be altogether ten notes, most of which are partially written.

His work was of the very highest class: no mathematician ever set before himself a higher standard of excellence or more nearly reached perfection; he attacked only important problems that had real influence on the advance of the science, and the

writings of no mathematician could shew a higher average. Great and irreparable as was the loss caused by his death, it was yet some consolation to feel that what he had left formed a worthy memorial of his powers and of the brilliance of his genius, and that his permanent place in the very highest rank of mathematicians was assured to him.

Professor CAYLEY endorsed every word Mr Glaisher had said: it was impossible to speak in too high terms of the value of Professor Smith's work. His wonderful knowledge of the processes of the higher parts of the Theory of Numbers shewed itself in everything he did. His work was of the very highest quality and excellence, and he could not too strongly express his sense of the great loss caused by his death.

The following communications were made to the Society:

(1) *A new form of equation of the* 16-*nodal quartic surface.* By Prof. CAYLEY*.

It was shewn that if x, y, z, ξ, η, ζ, be homogeneous linear functions of four co-ordinates, subject to the identical relations

$$x+y+z+\xi+\eta+\zeta=0,$$
$$ax+by+cz+f\xi+g\eta+h\zeta=0,$$

where $af=bg=ch=1$, then $\sqrt{x\xi}+\sqrt{y\eta}+\sqrt{z\zeta}=0$ is the equation of a quartic surface having the sixteen singular tangent planes (each touching it along a conic).

$$\begin{array}{ll} x=0,\ y=0,\ z=0, & \xi=0,\ \eta=0,\ \zeta=0, \\ x+y+z=0, & ax+by+cz=0, \\ \xi+y+z=0, & f\xi+by+cz=0, \\ x+\eta+z=0, & ax+g\eta+cz=0, \\ x+y+\zeta=0, & ax+by+h\zeta=0, \end{array}$$

$$\frac{x}{1-bc}+\frac{y}{1-ca}+\frac{z}{1-ab}=0,\ \frac{\xi}{1-gh}+\frac{\eta}{1-hf}+\frac{\zeta}{1-fg}=0.$$

(2) *On the mean radius of coils of insulated wire.* By Lord RAYLEIGH.

In electrical work it is often necessary to use coils of such proportions that their constants cannot well be obtained from the data of construction, but must be determined by electrical comparison with other coils whose proportions are more favourable. A method for comparing the galvanometer-constants of two coils, i.e. of finding the ratio of magnetic forces at their centres when

* This paper is published in full in the *Journal für die reine und angewandte Mathematik*, Bd. 94. "On the bitangents of a plane quartic."

traversed by the same current, is given in Maxwell's *Treatise*, Vol. II. § 753.

I have used a slight modification of Maxwell's arrangement which is perhaps an improvement, when the coils to be compared are of copper and therefore liable to change their resistance pretty quickly in sympathy with variations of temperature. The coils are placed as usual approximately in the plane of the meridian so that their centres and axes coinçide, and a very short magnet with attached mirror is delicately suspended at the common centre. If the current from a battery be divided between the coils, connected in such a manner that the magnetic effects are opposed, it will be possible by adding resistance to one or other of the branches in multiple arc to annul the magnetic force at the centre, so that the same reading is obtained whichever way the battery current may circulate. The ratio of the galvanometer constants is then simply the ratio of the resistances in multiple arc.

To obtain this ratio in an accurate manner, the two branches already spoken of are combined with two other resistances of german silver, so as to form a Wheatstone's balance. Of these resistances both must be accurately known, and one at least must be adjustable. The electro-magnetic balance is first secured by variation of the resistance associated with one of the given coils which resistance does not require to be known. During this operation the galvanometer of the Wheatstone's bridge is short-circuited. Afterwards the galvanometer is brought into action and the resistance-balance is adjusted. The ratio of the galvanometer-constants is thus equal to the ratio of the german silver resistances. The two adjustments may be so rapidly alternated as to eliminate any error due to changes of temperature in the copper wires. Indeed, if desired, the final tests of the electro-magnetic and resistance-balances might be made simultaneously.

If the ratio of galvanometer-constants be the final object of the measurement, there is nothing more to be done; but if we desire to know the ratio of the mean radii of the coils we must introduce certain small corrections for the finite dimensions of the sections. In the first place, however, it will be desirable to consider a little more closely what should be understood by the *mean radius* of a coil.

In Maxwell's treatment of the subject (§ 700) the mean radius of a coil is considered to correspond with the geometrical centre of its rectangular section, that is to say, the windings are assumed to be uniformly distributed over the section. In practice absolute uniformity is not attainable, and it is therefore proper to take into account the effect of a small imperfection in this respect. The *density* of the windings, i.e. the number of windings per unit area,

may be denoted by ρ, and is to be regarded as approximately constant.

The introduction of the factor ρ makes but little difference in the investigation of § 700. If we take the origin of co-ordinates x and y, no longer at the geometrical centre, but at what may be called the centre of density of the section, we shall have (as in the ordinary theory of the centre of gravity)

$$\iint \rho x dx dy = 0, \qquad \iint \rho y dx dy = 0,$$

the integrations being extended over the area of the section. If P be any function of x and y, $\bar{P}$ the mean value of the function (with reference to ρ), P_0 the value at the origin, we have

$$\bar{P} \iint \rho dx dy = \iint P \rho dx dy$$

$$= P_0 \iint \rho dx dy + \tfrac{1}{2} \frac{d^2 P}{dx_0^2} \iint \rho x^2 dx dy + \frac{d^2 P}{dx_0 dy_0} \iint \rho xy dx dy$$
$$+ \tfrac{1}{2} \frac{d^2 P}{dy_0^2} \iint \rho y^2 dx dy,$$

the terms of the first order disappearing in consequence of the choice of origin. In the terms of the second order we may neglect the effect of variable density, and write

$$\iint \rho x^2 dx dy = \tfrac{1}{12} \xi^2 \iint \rho dx dy,$$
$$\iint \rho y^2 dx dy = \tfrac{1}{12} \eta^2 \iint \rho dx dy,$$
$$\iint \rho xy dx dy = 0,$$

ξ, η being the breadths in the directions of x and y of the rectangular section. Thus

$$\bar{P} = P_0 + \tfrac{1}{24} \left(\xi^2 \frac{d^2 P}{dx_0^2} + \eta^2 \frac{d^2 P}{dy_0^2} \right).$$

The *form* of this expression is the same as when the windings are supposed to be distributed with absolute uniformity, but the mean radius and mean plane are to be reckoned with reference to the density of the windings.

In the application to the galvanometer-constant of a coil, we have, if A be the mean radius, ξ the radial and η the axial dimension of the section,

$$G_1 = \frac{2\pi}{A} \left(1 + \tfrac{1}{12} \frac{\xi^2}{A^2} - \tfrac{1}{8} \frac{\eta^2}{A^2} \right),$$

by means of which, ξ and η being approximately known, G_1 may be inferred from A, *or conversely* A may be inferred from G_1. If the ratio of galvanometer-constants of two coils has been determined by the electrical process, the ratio of mean radii can be accurately deduced by use of the above formula.

When the mean radius of a coil has been determined in this manner by comparison with another of proportions more favourable for calculation from the data of construction, other quantities relating to the coil may be deduced by mere calculation. For instance, the important constant g_1, denoting the mean area included by the windings, is connected with the mean radius A by the equation

$$g_1 = \pi A^2 + \tfrac{1}{12}\pi\xi^2.$$

A more direct process for determining g_1 electrically is given by Maxwell § 754, and has recently received an important application in the hands of Kohlrausch. In this method the quantity sought is proportional to the cube of a distance not very easy of precise measurement; and it is possible that the less direct method explained above may be the more accurate in practice.

(3) *On the invisibility of small objects in a bad light.* By Lord Rayleigh.

In a former communication to the Society (March 6, 1882) I made some remarks upon the extraordinary influence of apparent magnitude upon the visibility of objects whose 'apparent brightness' was given, and I hazarded the suggestion that in consequence of aberration (attending the large aperture of the pupil called into operation in a bad light) the focussing might be defective. Further experiment has proved that in my own case at any rate much of the effect is attributable to an even simpler cause. I have found that in a nearly dark room I am distinctly short-sighted. With concave spectacles of 36″ negative focus my vision is rendered much sharper, and is attended with increased binocular effect. On a dark night small stars are much more evident with the aid of the spectacles than without them.

In a moderately good light I can detect no signs of short-sightedness. In trying to read large print at a distance I succeeded rather better without the glasses than with them. It seems therefore that the effect is not to be regarded as merely an aggravation of permanent short-sightedness by increase of aperture.

The use of spectacles does not however put the small and the large objects on a level of brightness when seen in a bad light, and the outstanding difference may still be plausibly attributed to aberration.

February 26, 1883.

MR GLAISHER, PRESIDENT, IN THE CHAIR.

The following were balloted for and duly elected Fellows of the Society:

Rev. R. Appleton, M.A. Trinity College.
Mr R. F. Scott, M.A. St John's College.
Mr M. J. M. Hill, M.A. St Peter's College.
Mr W. A. Bond, M.A. St John's College.
Mr J. Larmor, B.A. St John's College.

The following communications were made to the Society:

(1) *The original function of the canal of the central nervous system of Vertebrata.* By A. SEDGWICK, M.A.

The central nervous system of all known animals with certain doubtful exceptions, arises from the epiblast. The region of the epiblast from which it arises may either persist in the adult as part of the superficial epidermis, or it may be pushed in so as to give rise to a tube, from the walls of which the central nervous system is developed. The last mentioned method is characteristic of the Vertebrata. The walls of this tube become differentiated into a superficial epithelial layer lining it, and an external mass of nervous matter. The tube persists as the canal of the nervous system; the epithelium lining it becomes the ciliated epithelium of this canal, which therefore corresponds to the external epithelium of the body-wall.

I may here draw attention to the fact that the vertebrate stock must have separated from that of other animals before the nervous system was separated from the external epithelium of the body; that in fact the vertebrate nervous system *never is separated by any ingrowth of mesoblast from the superficial epiblast* from which it arose; as is the case in all but the most primitive of the Invertebrata. This superficial epiblast in Vertebrata is involuted and gives rise to the ciliated epithelium just mentioned of the central canal. Three stages may be distinguished in the development of this canal, and I suppose that all three have had a functional counterpart in the evolution of the organ.

In the first stage a groove extended along the whole length of the middle dorsal (or ventral?) line of the body, the nervous system being placed in the deeper layers of the epidermis of this groove. This stage I propose to call the *groove stage.*

In the second stage, which may be called the *siphon stage*, the groove had become converted into a canal, open in front at or near the anterior end of the body, and open behind close to the anus.

In the third stage the canal has completely closed; this is the present stage.

There have been doubtless many other stages, each with its special functional importance in the evolution of the neural canal; but those which I have just mentioned are most obviously suggested by the ontogeny of this organ.

Of the existence of the groove stage there can I think be no doubt. It has left its mark in the medullary groove of Vertebrates; and indications of the occurrence of a similar structure are to be found in the embryos of Annelida and Arthropoda. The ventral groove of Neomenia and Proneomenia is probably the same structure persisting in the adult.

This groove was probably richly ciliated. Once established it soon became deeper.

The function of the groove was in my opinion partly respiratory and partly protective. As the nervous system increased in thickness, the deeper parts of it became so far removed from the surface, that the supply of oxygen which reached them was inadequate. This would be remedied by an increase of the surface over which the nervous system was spread, and this increase might be produced by enlarging the superficial area over which the central nervous system extended. It is improbable that this should happen, because the tendency of evolution seems to have been to localise the central nervous system, probably for the sake of its greater security. The increase of surface required seems to have been obtained by the development of this groove, and the deeper the groove became the more completely would it serve its double function. Thus the way was prepared for the siphon stage which proceeded from the groove stage, by the conversion of the groove into a canal open at either end. The neural canal therefore owed its origin to the requirements of protection and respiration. When once formed it must have continued to discharge some function, otherwise it would have atrophied, as we know is the habit of useless structures. The function of the canal at this stage of its evolution is the subject of the present discussion.

The relations of the neural canal at the siphon stage, which is well marked in the development of Ascidians and Amphioxus, are well known to all students of embryology. It is open behind into the hind end of the alimentary canal and in front, in the cephalic region of the body.

How it acquired its opening into the alimentary canal is perhaps hard to understand. The discussion of this question involves the discussion of a still more difficult question, viz. the relation of the permanent anus to the blastopore; this I reserve for a subsequent occasion. I may however point out here that

development points to the fact that the blastopore was placed within the medullary plate, and that therefore on the conversion of the medullary groove into a canal, the alimentary canal would open into the hind end of that canal, and the two tubes would open to the exterior together.

It is quite clear that the anus of existing vertebrata is not in the position of the primitive anus or blastopore of ancestral forms, and it has been commonly supposed that the present anus is a new formation. That the blastopore closes is certain, but it has been recently pointed out by Mr Weldon[1] that the present anus occurs along the line of the blastopore: and I hope soon to be able to show that the permanent anus is identical with the blastopore, the temporary closure of which is simply a matter of developmental necessity. However this may be, there can be but little doubt that this relation of the hind end of the neural canal to the alimentary canal has existed in the ancestors of vertebrates.

To return to the main question, What is the function of the neural canal at this stage? It seems to me that that function must have been in the main a respiratory one. The water entered the canal by the anterior pore, was driven through it by the cilia, and at the hind end passed through the neurenteric canal into the alimentary canal, and so out by the anus. In support of this I appeal to certain well-known physiological and anatomical facts.

In the first place, in the vertebrata the brain requires more oxygen for its well being than any other tissue of the body, and in those vertebrates, *e.g.* Amphibia and Sauropsida, in which there is only one ventricle, special arrangements are present to ensure a supply of pure arterial blood to the head. In the second place, in the tracheate animals the central nervous system has a specially rich supply of tracheæ. Finally in certain worms, *e.g.* Nemertines, Aphrodite, the whole nervous system contains hæmoglobin, which may be supposed to exercise a special attraction for oxygen, and hold it in a convenient state for the use of the nerve-cells.

It is interesting to notice here that in most of the animals I have just mentioned, in which there are special arrangements for the respiration of the nervous system, the vascular system is but little developed. It seems probable that the ancestral vertebrate with the siphon stage of neural canal was without a well-developed vascular system. When this and definite respiratory organs became developed, a new stage in the evolution of the neural canal was reached, in which it lost its respiratory function, this being assumed by the vascular system. As a result of this, the anterior and posterior opening became closed. This brings us to the

[1] *Quart. J. of Mic. Science*, 1883.

present condition of a closed central canal, whose function it is not within my province to examine.

Before concluding I wish to point out that a canal leading into the centre of the central nervous system, is not confined to the vertebrata. Such canals are present leading into the cephalic ganglia of adult Nemertines, and in the most conspicuous part of the nervous system of Balanoglossus, a similar arrangement of a very complicated nature exists[1]. They are also present in the cephalic ganglia of the embryos of most tracheate animals.

The cephalic pits in the tracheata served, according to my view, for respiration in the aquatic ancestors of the living forms. The development of tracheæ is not at present known, but I think, that if there is any truth in the hypothesis of the original function of these tracheate cephalic pits, it will be found that the trachea of the cerebral ganglia arise from the epiblastic cells lining these pits.

(2) *On a new microtome, designed to increase the accuracy and rapidity of section cutting.* By W. H. CALDWELL, B.A.

(3) *On the development of the Pelvic Girdle and Skeleton of the Hind Limb in the Chick.* By ALICE JOHNSON, Newnham College.

The following paper contains a summary of the conclusions arrived at by a study of the development of the pelvic girdle and hind limb of the Chick. The investigation was undertaken at the suggestion of the late Prof. Balfour, with the view of testing the comparison which Marsh had instituted between the pelvis of Dinosaurs and that of Birds. I hope to publish later a full account—with figures—of my results.

In the Chick, as in the Elasmobranch, the skeleton of the hind limb is developed continuously with the pelvic girdle, the parts of the girdle which are nearest to the skeleton of the limb are first developed, and the dorsal and ventral elements appear later, so that the girdle may be said to be an outgrowth from the femur.

The chief points which will be considered here are the relations of the pubis in the embryo, and the conclusions which may be drawn from thence as to the homologies of the pubis in Birds with that in other types.

Both the pubis and ischium, as Bunge[2] has described, are at first placed with their long axes at right angles to the long axis of the ilium, and afterwards rotated backwards, so as to occupy the position in which they are found in adult Birds, where the long axes of all three bones are parallel to one another. Bunge and other writers following him have asserted that the pubis is at first

[1] From an unpublished research by Mr W. Bateson, of St John's College.

[2] A. Bunge, "Untersuchungen zur Entwickelungsgeschichte des Beckengürtels der Amphibien, Reptilien und Vögel." Dorpat 1880.

developed independently, and Bunge states that it fuses with the rest of the girdle on about the eighth day of incubation. I find that all the elements of the girdle are continuous with one another at the earliest and all other cartilaginous stages. It is the ossification alone which gives rise to any want of continuity in any part of the girdle.

On the sixth day, when the pubis can first be recognised as a distinct element, it consists of two branches, an anterior and a posterior. The former is directed forwards and very slightly outwards; the latter is rather longer, though no broader than the former, and is directed vertically downwards. In later stages, the anterior branch grows less than the other elements, so that it gradually comes to occupy a quite subordinate position. It forms the pectineal process of the pubis in the adult. In some birds it is absent, while in others, e.g. Geococcyx and Apteryx, it is comparatively well developed. It is undoubtedly homologous with the pectineal process of the pubis found in many, especially some of the lower Mammals. In Ornithorhynchus this process reaches the proportionate dimensions found in the embryo bird (see fig. 4).

Fig.1.

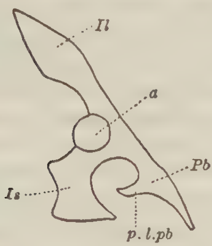


Fig.2.

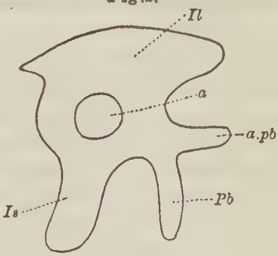


Fig.3.

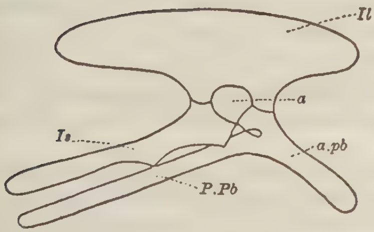
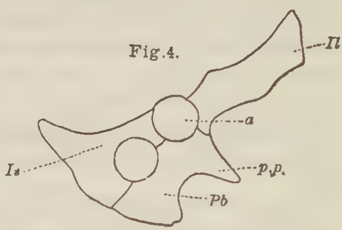


Fig.4.



Explanation of figures.

Fig. 1. Pelvic girdle of *Cyclodus*. Fig. 2. Pelvic girdle of young Chick. Fig. 3. Pelvic girdle of *Laosaurus*. Fig. 4. Pelvic girdle of *Ornithorhynchus*.

Il. Ilium. Is. Ischium. Pb. Pubis. a. acetabulum. p. l. pb. processus lateralis pubis. a. pb. anterior branch of Pubis. P. Pb. posterior branch of Pubis ('Post-pubis' of Marsh.) p.p. pectineal process of Pubis.

From a consideration of the adult forms, Marsh[1] has compared the pubis of birds to the 'post-pubis' of some of the Dinosaurs described by him, while he views the pectineal process of the pubis as being homologous with the pubis of the same Dinosaurs and with the pubis of Reptiles.

In Laosaurus, the embryo bird and Ornithorhynchus (see figs. 3, 2, 4), the various pubes are so similar in their double nature and the almost equal development of both branches, that I think there can be no doubt that Marsh has described the homologies correctly in these cases. In Reptiles, the homologies are not equally obvious. In them also, however, we find a double pubis. In Chelonia its two branches are well developed. Both point forwards, and one is directed inwards towards the symphysis, while the other passes outwards. The latter is called the processus lateralis pubis. It is found again in Lizards, but here it is more variable. It is given off from about the middle of the outer surface of the pubis. Generally it is directed downwards and outwards, but sometimes it points directly backwards, as in Cyclodus (see fig. 1). The pubis itself is directed forwards and inwards to the symphysis. In a few Lizards, and in Crocodiles, the processus lateralis is absent.

In comparing the two parts of the pubis in Reptiles with those in Dinosaurs, Birds and Mammals, there are two views which most obviously present themselves. Either (1) the processus lateralis pubis is the pectineal process of the pubis, and the so-called pubis is homologous in all cases, or (2) the processus lateralis becomes the pubis of the higher forms, and the reptilian pubis becomes the pectineal process. It seems to me that the relations of the bones in Dinosaurs prove the greater correctness of the latter view. Marsh[2] divides the Dinosaurs into four classes, in two of which no post-pubis is present, but the pubis passes forwards and inwards, forming the symphysis, and thus evidently corresponding to the reptilian pubis. In the other two classes, the pubis is divided into two branches. The posterior of these ('post-pubis' of Marsh) points backwards and thus resembles the pubis of birds, while the anterior ('pubis' of Marsh) is directed forwards and inwards, and thus, though it does not form a symphysis, may be compared to the pubis of the other Dinosaurs, and consequently to the reptilian pubis.

From this point of view, then, we see the processus lateralis pubis first assuming a posterior position in such lizards as Cyclodus, and becoming more important in Dinosaurs, till in adult Birds and Mammals it takes the place of a functional pubis, and in Mammals goes so far as to form a symphysis with its fellow. Meanwhile the

[1] O. C. Marsh, "Principal Characters of American Jurassic Dinosaurs." *Amer. Journal of Science*, 1878 and 1879.

[2] O. C. Marsh, "Classification of Dinosaurs." *Amer. Journal of Science*, 1882.

part corresponding to the reptilian pubis has dwindled down into the pectineal process of the pubis in the adult forms of the higher types. These results may be tabulated as follows :—

Reptiles	*Dinosaurs*	*Embryo Bird*	*Birds*	*Mammals*
1. Pubis	1. Anterior branch of Pubis ('Pubis' of Marsh).	1. Anterior branch of Pubis.	1. Pectineal process of Pubis.	1. Pectineal process of Pubis.
2. Processus lateralis pubis.	2. Posterior branch of Pubis ('Post-pubis' of Marsh).	2. Posterior branch of Pubis.	2. Pubis.	2. Pubis.

The skeleton of the hind limb is at first represented by a single axial mass, extending for about half the length of the limb. In the next stage, all the chief elements of the skeleton—Humerus, Tibia, Fibula, Tarsus, and five distal cartilages—are apparent, but perfectly continuous with one another. Later, the cartilaginous centres appear. Of these, there are three in the tarsus, two in the proximal and one in the distal segment. The two proximal, the Tibiale and Fibulare, fuse first with one another, and later with the Tibia, while the distal segment fuses with the metatarsals. The five distal cartilages of the limb grow and segment, giving rise to the phalanges of the digits, as well as to the metatarsals. The first metatarsal is at first continuous with the tarsus, but afterwards splits off from it and is carried downwards with the growth of the foot, till it lies at some distance from the tarsus.

At the end of last year Baur[1] published an account of the tarsus of Dinosaurs and the development of the Bird's tarsus. With regard to the latter point, my own results (which were completed before I had seen his paper) agree with his in every detail, except that he describes the first metatarsal as originating at a distance from the tarsus, and never coming into any connection with it. My investigations into the development of the pelvic girdle also fully bear out his conclusions as to the close relation existing between Birds and Dinosaurs.

(4) *On the nitrogenous reserve-materials in parts of plants other than seeds.* By M. C. POTTER, B.A.

It has long been known that the organs of plants which serve as depositories of reserve material, contain considerable quantities of nitrogenous organic substances. In the case of seeds, it has been ascertained that the nitrogenous organic substance is of the nature of proteid, for the most part deposited in the form of definite grains termed Aleurone grains, but our information con-

[1] G. Baur, "Tarsus der Vögel und der Dinosaurier." *Morphologisches Jahrbuch.* Band 8. Heft 3. 1882.

cerning the nature and form of organic nitrogenous substances in other depositories is incomplete.

With a view to determine the form in which nitrogenous organic substances are deposited in Bulbs, Buds, Rhizomes, and Roots, and to ascertain if grains similar to Aleurone grains are deposited in these, I have examined the following cases.

First, Buds:

Fagus Sylvatica.
Vitis Vinifera.
Convallaria Majalis.
Pyrus Aucuparia.
Acer Pseudo Plantanus.

And the buds of a species of

Corylus.
Pyrus.
Crategus.
Magnolia.
Alnus.

In these I have been unable to find any trace of granular nitrogenous deposits, though often considerable quantities of Starch were found deposited, and the young meristematic cells were quite full of contents.

Second, Rhizomes:

Equisetum Arvense.
Epilobium Hirsutum.
Acorus Calamus.
Polygonatum Vulgare.
Pteris Aquilina.
Rumex (species of).
Iris Tuberosus.
Ophioglossum Vulgatum.

Third, Tubers and Corms:

Ranunculus Ficaria.
Bunium Flexuosum.
Arum Maculatum.
Colchicum Autumnale.
Gymnademia Canopsia.
Dendrobium Cannæfolia.
Cœlogyne Cristata.
Helianthus Tuberosus.
Phajus Grandiflora.
Crocus.

In both these cases again I was unable to find any proteid granules or crystalloids. Since crystalloids have been discovered by Cohn in the tuber of the Potato, I hoped to find similar bodies in *Ranunculus Ficaria* and *Bunium Flexuosum*, but was disappointed; but it must be mentioned the latter had already germinated and produced a green leaf.

Fourth, Bulbs:

Scilla Nutans.
Tulipa Suaredens.
Galanthus Plicatus.
Galanthus Nivalis.
Alium Stramonium.
Hyacinthus Amethystinus.
Muscari Comosum.
Narcissus Poeticus.
Narcissus Incomparabilis.
Narcissus Pseudo Narcissus.

In none of these, except *Narcissus Poeticus*, could I find any proteid granules. But in a bulb of *Narcissus Poeticus* which had been preserved in methylated spirit, in that part of the bulb which represents the stem I found numerous granular bodies, more or less spherical in form, which easily dissolved in potash solution, were insoluble in ether, alcohol, acetic acid or in solutions of sodic chloride, either when sections containing them were immersed directly they were cut in ether, or when first placed directly in absolute alcohol; they also stained orange yellow with iodine.

From these reactions I conclude they are granules of proteid, and from their insolubility in solutions of sodic chloride or acetic acid, that they are proteids of the albuminate type. The granules are of relatively large size, and it appears that only one occurs in each cell. They consist of an outer hyaline and an inner opaque part, and it was this latter which dissolved in dilute potash.

In section stained with hæmatoxylon, it is often possible to see the nucleus stained and in its place, side by side with the proteid granule. In order to assure myself that these granules had not been formed in consequence of the action of the methylated spirit, I examined fresh specimens, and found that the granules were present. In the fresh state they appeared to be homogeneous, and dissolved entirely in dilute potash, and stained rather more deeply with iodine than those which had been kept in methylated spirit. In order to see if similar proteid granules occurred in other bulbs, I examined the bulbs of the plants mentioned in the list given above, but I failed to find them. Unfortunately, owing to the fact that the observations upon *Narcissus Poeticus* had been made late in

the winter, the other bulbs had begun to grow before I was able to examine them, and this may account for the absence of granules in them, since the granules in the Narcissus bulb disappear soon after the bulb has commenced to grow.

March 12, 1883.

Mr Glaisher, President, in the Chair.

Mr M. C. Potter, B.A., St Peter's College, was balloted for and duly elected a Fellow of the Society.

The following communications were made to the Society:

(1) *On certain points in the anatomy of Brachiopods.* By W. H. Caldwell, B.A.

The knowledge of the structure and life history of the genus Phoronis led me to put forward a new interpretation of the body plan of Brachiopoda[1]. To test this I have examined species of the following genus, viz. Lingula, Discina, and Crania among Ecardines, and Argiope, Megeilea and Rhynconella among Testicardines. In the present note I propose to trace some of the modifications in structure occurring in these forms. I shall confine myself to (i) the nervous system, (ii) the body cavity and mesenteries.

Nervous system.

The body wall of all Brachiopoda has a basement membrane lying immediately below the ectoderm.

The fact that this lamella passes into the septum dividing the body cavity of the præoral lobe from that of the trunk, suggests a mesoblastic origin. In all the Brachiopoda I have examined, the central nervous system lies outside this basement membrane.

Dr Van Bemelen has recently described[2] the central nervous system as partly imbedded in this basement membrane. The forms he studied were Terebratula vitius and Waldheimia Cranium.

In both these species, as well as in Lingula Disima and Rhynconella, very definite nervous tracts are present. The ectoderm over the greater part of the body has only a few kinds of cells. In these forms there is a complete circumœsophageal nerve ring, and various nerves passing off from this. These peripheral nerves have however no constant position in the different genera. In the smaller species I have failed to trace the supraœsophageal part of the ring round the mouth. But in

[1] *Proc. Roy. Soc.* Dec. 1882. [2] *Jenaische-Zeit.* Dec. 1882.

all Brachiopods by far the most important concentration of nervous elements is below the mouth. To what extent the smaller Brachiopods possess a continuous nervous sheath representing the ancestral condition, I have not been able to determine. It is possible that they are descended from larger species, and really further removed from the ancestor than the latter. If this be so, they present analogy to Polyzoa. The variation in the position of the concentrations of the nervous system already mentioned, proves that the ancestor of the present Brachiopods possessed a continuous ectodermic nervous sheath. It is possible that this ancestor descended from forms with the adult nervous system inside the mesoblast.

The fact that no evidence for this exists does not render it improbable. *If any organ is arrested in its development, and this stage persists throughout life, there can be no ortogenetic evidence of its more highly specialised ancestor.*

If the larvæ of Brachiopoda possess a ganglion in the præoral lobe, it is probable that this, as in Phoronis, disappears at the close of free swimming life.

I have this opinion on the fact, that no trace of a structure resembling the brain of Chætopods and Molluscs is present in adult Brachiopoda.

Body cavity and Mesenteries.

The development of the mesoblast in Brachiopoda has unfortunately not been traced. A knowledge of the fate of the archenteric diverticula of Argiope would probably settle many problems. I therefore put forward the following interpretation only tentatively.

In Phoronis there is a ventral mesentery, attaching the gut to body wall. The dorsal mesentery breaks down as soon as it is formed. This is probably secondarily induced by the adult approximation of mouth and anus.

In Brachiopoda there is a mesentery with dorsal relations. Its existence may be due in part to the dorsal surface not being so seduced. In crania the anus is almost terminal.

The gastro- and ileo-parietal bands of Brachiopods have been regarded as transverse septa, dividing the body into three segments. They do not however divide the body cavity in the same way as the septum cutting off the cavities in the arms from the throat in the trunk does. They are *lateral* mesenteries passing from the side of the first and second stomach to the walls of the body. I would suggest that they are parts of the same pair of lateral mesenteries homologous with that of Phoronis. Their adult position in two planes almost parallel to each other is explained by the bending of the gut upon itself.

(2) *On the vascular system of Pelophilus (Boa) madacascariensis.* By Dr H. GADOW.

The author made a communication on the vascular system of a large specimen of Madagascar Boa. He chiefly discussed certain points in the veinous system. These require further investigation which he has in hand, and the results of which he hopes to communicate to the Society in a more complete form.

April 30, 1883.

MR GLAISHER, PRESIDENT, IN THE CHAIR.

Mr R. H. SOLLY was elected an Associate of the Society.

The following communications were made to the Society:

(1) *On the use of a collimating eye-piece in spectroscopy.* By Professors LIVEING and DEWAR.

We seem to owe to von Littrow the first suggestion for a spectroscope with only one telescope which should serve at once both as collimator and observing telescope; but hitherto for the most part such instruments have been abandoned on account of the embarrassment caused by reflexions from the surfaces of the object-glass. Professor Brackett (*American Journal of Science*, July, 1882) mentions such an instrument constructed for the College at Princeton, and says that it was necessary to put a patch on the object-glass to intercept reflected light, although it had been specially constructed with curvatures calculated to reduce the inconvenient reflexions to a minimum.

Professor Mendenhall, in the Memoirs of the Science department of the University of Tokio, Japan, has called attention to the advantages of a collimating eye-piece in grating spectroscopes.

The most obvious of these are (1) the facility with which a reflecting grating can be adjusted so that its plane may be perpendicular to the axis of the collimator if it is to be used in that position; and (2) the facility with which the distance of the slit from the object-glass of the collimator may be adjusted so that the light may be incident on the grating in a parallel beam. This condition has to be fulfilled in order to obtain the best definition whenever the telescope and collimator are not symmetrically placed with reference to the grating, that is to say, if a reflecting grating is used, in all ordinary cases. He points out however that when the axes of the telescope and collimator are equally inclined to the normal plane of the grating, which corresponds to the case of a

prism in the position of minimum deviation, the definition is not dependent on the exact parallelism of the incident beam. This position of telescope and collimator is obviously attainable, when a reflecting grating is used, only when they are either identical or placed one above the other in the same vertical plane. We have not met with any precise account of the Princeton instrument. A collimating eye-piece with a plate of glass, or quartz, as reflector, while allowing the collimator and telescope to be absolutely identical, has the disadvantage of much loss of light, and we preferred to make the axes of collimator and telescope slightly inclined to one another in the same vertical plane, but at such a small angle that one object-glass and tube serves for both. We use in fact only the upper half of the slit for the admission of light, and reflect the return beam by a right-angled prism placed close to the slit just below the centre. The edges of this prism are of course short and its upper bounding plane passes through the axis of the tube of the telescope. The length of the sides is about $\frac{3}{4}$ inch in order to give a sufficiently large field of view. When the source of light is large enough to fill the whole of the upper half of the slit the prism intercepts part of the light which would otherwise fall on the lower part of the object-glass, but no practical inconvenience results from that. There is no loss of definition because there is lateral symmetry in the incidence of the light on the object-glass. A stop with a small hole is placed immediately behind the reflecting face (the hypotenuse) of the prism to intercept stray light from the edges of the slit which might otherwise reach the eye.

The reflexions from the surfaces of the object-glass we have found less embarrassing than other observers seem to have found them. One reason is that the tube is lined throughout with black velvet. We have long since found the necessity of this when observing either extremity of the spectrum, as ordinary blacking and stops will not prevent a good deal of stray light entering the eye. Another reason is that we use a single plano-convex quartz lens as object-glass, so that there are but two surfaces to reflect any light. The plane surface produces a well-defined image of the slit which can be very well seen when the eye-glass is removed. It is however a distant object and cannot be seen through the eye-glass at all. It is quite intercepted by a very small patch on the centre of the object-glass, but this is not really needed, for when a strong light is used the slit can be made narrow and the reflected light reduced. The curved surface of the object-glass disperses the light it reflects more than the plane surface, so that more of it is absorbed by the sides of the tube.

It is easy to throw a strong light into the slit in such a direction as to give very inconvenient reflexions, but the proper

direction is as easily attained by means of a lens in front of the slit fitted into a tube which is a prolongation of the telescope tube. When the source of light is so placed that the image produced by the lens is just above the centre of the slit the best results are obtained.

Prisms may be used as well as gratings with the instrument if the train terminate with a half prism with its face silvered, and we have tried such a plan with success. The dispersion is of course double what it would be with the same prisms used in the ordinary way, as the light passes twice through them. The light is however enfeebled though the dispersion is increased, and the gain in the use of prisms appears to be less than it is in the use of gratings where the only extra cause of loss of light is the reflecting prism which makes but a very trifling difference.

The advantages which we think we attain by this method are:

1. Great dispersion without loss of definition.

The ordinary formula connecting the wave-length, grating space, and inclinations of the collimator and telescope to the normal is $a(\sin\theta + \sin\phi) = n\lambda$, where a is the space between successive lines of the grating, θ, ϕ the inclinations of the axes of the collimator and the telescope respectively to the normal, n the order, and λ the wave-length.

In ordinary instruments the difference between θ and ϕ must be something considerable, and for the spectra of the higher orders either the incident or the diffracted ray is very oblique and the brightness as well as the definition suffers. For any particular position of the grating with reference to the collimator the angular separation of two rays in the field of view will be measured by $d\phi/d\lambda$, which is proportional to $n \sec\phi$, and the maximum value of n is obtained by making both θ and ϕ as large as possible. It will therefore happen frequently, when both θ and ϕ can be made as near $90°$ as we please, that we are able to observe a particular ray in a spectrum of a higher order than when there must be a considerable difference between them. The definition is also as good as it can be made for any given grating because the eye-piece ensures the parallelism of the incident rays. As a test we may mention that with Rowland's grating having 14,438 lines to the inch the less refrangible of the E lines is very distinctly divided in the 5th order, though we were not able to resolve this line with a Rutherfurd grating having 17,296 lines to the inch used in the old way, notwithstanding that this latter grating is in some respects the better of the two. The E group in the 5th order bears the magnification of a Ramsden eye-piece, with lenses of one inch focal length, very well. This is the highest power we have used; and it will be seen

that the magnification is independent of the object-glass of the telescope in this case. With the 6th order no more can be seen though the lines are wider apart. It will be observed that for a given inclination of the telescope to the normal the dispersion with the collimating eye-piece is just double what it is when there is a separate collimatoı placed normal to the grating, for in the former case we have $n\lambda = 2a \sin \phi$ and in the latter $m\lambda = a \sin \phi$, so that $n = 2m$. Against this we must set the disadvantage of the increased number of overlapping spectra, the most troublesome inconvenience attending the use of gratings. The uncorrected lenses which we use for the sake of observing the ultra-violet rays, are an advantage in regard to the overlapping spectra, because the parts of two spectra which are in the field of view at the same time are not in focus together. Thus if the image of the sun, or other source of light is projected on to the slit with a lens of short focus, the spectrum when in focus forms a narrow but bright band, whereas the overlapping spectrum which is out of focus forms a much broader and consequently less bright band. This weakening of the overlapping spectrum is sufficient when the brighter parts of the solar spectrum are under observations, but not sufficient when either extremity of the spectrum has to be observed against an overlapping bright part. In such cases we have used a coloured glass or liquid to cut off the brightest light. Where the spectrum is more discontinuous than that of the sun or electric arc the overlapping spectra are not so frequently in the way.

2. Facility and accuracy in measurement of wave-lengths.

The method of measuring wave-lengths commonly adopted. namely, to fix the grating perpendicular to the collimator and read the deviation of the ray in the corresponding orders of spectra on the two sides, leaves little to be desired in point of accuracy, inasmuch as the errors of adjustment affect the readings on the two sides in opposite ways and so are compensated in the result. By the use of the collimating eye-piece equal accuracy is obtained by readings on one side only. This is owing in the first place to the facility and accuracy with which the zero reading is obtained. The collimator being fixed and the grating moved the zero reading is taken by placing the grating so that the image of the slit formed by direct reflexion from the grating may be on the pointer or cross wires in the eye-piece. The readings of the ray to be measured may then be taken in the successive orders of spectra one after another. In the next place there are no instrumental errors of adjustment to be taken into account. It does not matter if the axis of the collimator or the plane of the grating do not pass accurately through the centre of rotation. All that is required is

the angle through which the grating is turned, that is to say the circle must be well divided. Readings on the two sides may still be taken and will help to eliminate errors of reading and errors due to inequalities of temperature and so on. For a given angular deviation the dispersion is double what it is in the old method which facilitates accurate reading; and it often happens that a spectrum can be observed of an order which involves a deviation greater than any which can be observed in the old method. Thus with a grating which when fixed perpendicular to the collimator will only shew the D lines in the first and second orders, it may be possible to observe them with a collimating eye-piece in the 5th order. Moreover gratings are usually very unequal on the two sides. The Rowland's grating we have used gives very feeble spectra in orders above the second on one side, bright spectra of those orders on the other side, and it is a great advantage to be able to use the bright side alone. By way of example of the accuracy easily obtainable by the use of the collimating eye-piece we may cite one determination. Readings were taken of the b group of magnesium lines in the 3rd, 4th, and 5th orders on both sides, that is six observations of each line, the circle being divided in 10′ and the vernier reading half-minutes. The mean of the two readings of the same line in the same order on the two sides did not in any case differ from the single reading by half a minute. The wave-lengths deduced from the whole set were

	b_1	b_2	b_4
	5183·14	5172·20	5166·62
Ångstrom's Nos.	5183·10	5172·16	5166·88

In comparing our numbers with those of Ångstrom it was of course necessary to determine the distance between the lines of the grating according to the standard of length employed by Ångstrom. This was done by observations of the deviations of the D and E lines in the spectra of a sodium flame and of a spark between iron points respectively, as well as in the solar spectrum. For spectroscopic purposes it appears to us to be far the best plan to adopt Ångstrom's standard of length, whether his millimetre be the most accurate attainable or not, and when we get a more exact determination of absolute wave-length, all numbers on Ångstrom's scale can be reduced to the new scale by multiplication into a constant factor. For the purpose of reducing the grating space to Ångstrom's scale Mendenhall has proposed the use of a rather feeble iron line below E, but it seems to us far better to use the mean of the two E lines, because this was Ångstrom's standard line on the measurement of which he bestowed the greatest pains; besides E_2 is a strong and

easily recognised line in the spark spectrum of iron. The D lines are convenient from the facility with which they may be observed, and though they are diffuse lines in the sodium flame they can be read accurately by the reversed line when the dispersion is high. The temperature correction is an important one whenever the observations are carried on in a place of variable temperature, a change of 4° in the temperature of the Rowland's grating we have used making a difference of more than half a minute in the deviation of the b group in the 3rd order. Mendenhall has determined the coefficient of expansion of one of his gratings by observations of the deviation at different temperatures, and his result agrees very nearly with the coefficient of expansion of speculum metal observed by Smeaton; and it is sufficient, with our present instrumental means, to correct the grating space for temperature by either coefficient.

It had occurred to us that instead of measuring directly the angle through which the grating is turned we might measure the deviation of the ray reflected from its surface and so double the angle to be measured and diminish errors of measurement. This method, however, does not answer so well as the other when the measures are taken on one side only, and the reasons are obvious. If the two telescopes are not very accurately adjusted to the centre of rotation sensible errors will affect the measured angles; and the zero reading cannot be obtained in the same way as the other readings but has to be found by placing the telescope opposite the collimator and taking the reading in that position. The method answers well if the readings are taken on both sides. The adjustment of the axis of the telescope to the centre is a troublesome business, and is liable to small but sensible errors in change of focus for the observation of different rays. We make the adjustment by first replacing the object-glass by a lens of short focal length so as to make the telescope into a microscope. A small plumb-line consisting of a single thread of unspun silk is then adjusted on the top of the instrument until it does not alter its place in the field of view when the turn-table or the telescope is rotated. The cross wires of telescope and collimator are then brought on to it. After this has been done the readings on the two sides differ so much that the double readings cannot be dispensed with. By taking the mean of the readings on the two sides very accurate results may be obtained, as both instrumental errors and errors of reading are reduced as much as possible. Thus three readings of the D lines on each side, with a Rowland's grating on which the distance between the lines had been gauged by readings of the solar lines E, gave the wave-lengths 5895·06 and 5889·05, numbers which differ from Ångstrom's by only ·07.

In order to photograph lines for the purpose of wave-length

measurement, we have a small photographic slide which holds a plate one inch long by half an inch wide. When in use this slide takes the place of that portion of the instrument which carries the pointer and eye-lenses. A sliding shutter exposes only the lower half of the plate which is exactly opposite the reflecting prism, and when the exposure of that half of the plate is completed, the plate which is held in a small drum is revolved through 180° about an axis perpendicular to its plane, and thus what was before the upper side becomes the lower. The second half of the plate is now exposed and thus two images of the line are impressed on the plate at equal distances on either side of the axis about which the plate was turned. This axis in fact takes the place of the cross wires or pointer as the point of reference; and the distance of the line from it has to be deduced from the distance between the two images of the line measured under a microscope by a micrometer, and has then to be reduced to arc and added or subtracted from the observed angle of inclination of the grating. Two sets of photographs of the strong magnesium line taken in this way gave the wave-length 2851·9 and 2851·7 respectively; the wave-length found by the old method being 2852·0. This line is always diffuse, so there is room for some error in the determination of its middle.

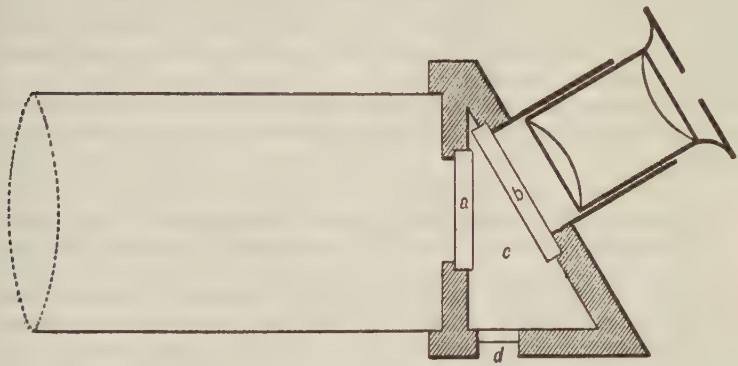
The eye-piece was constructed for us by Mr A. Hilger, who carried out our instructions very carefully.

(2) *On some modifications of Soret's fluorescent eye-piece.* By Professors Liveing and Dewar.

If the eye-piece of the spectroscope be removed and a plate of uranium glass substituted there is no difficulty in seeing, in a dark room, the brightest ultra violet lines by the fluorescence, and approximate measures of the deviation of them may of course be made. But as soon as you bring an eye-glass to bear on the image and try to take exact measures it becomes very tiresome. All extraneous light has to be excluded, and unless the slit is very wide the fluorescent light is in most cases so faint that it is barely possible to see the pointer without losing sight of the line to be observed.

In Soret's eye-piece the eye-glass is moveable about a vertical axis so that the fluorescent plate can be viewed obliquely from one side and so stray light coming down the tube of the telescope does not reach the eye. It occurred to us that it would be better to look down on the fluorescent plate obliquely from above. The spectrum and pointer would then be foreshortened in a vertical plane only and observation facilitated. Further, if the fluorescent screen were in form of a prism of small angle with one face perpendicular to the axis of the telescope and the other perpendicular to the axis of the eye-lenses, the best conditions would be secured

for seeing the fluorescent light while stray light would be refracted out of the line of vision. Eye-pieces on this plan have accordingly been made for us by Hilger, one with a thin wedge of uranium glass and others with a wedge-shaped vessel of which the side next the object-glass is a plate of quartz, and on the inner face of this the pointer is placed. The side of the vessel next the eye-lenses is of glass, and the vessel is filled with a solution of æsculine made slightly alkaline with ammonia. The body of the vessel is of brass, and there is a small window closed with glass at the bottom through which light may be thrown on the pointer when necessary.



The figure shews the eye-piece in section. *a* is the quartz plate, *b* a glass plate, *d* the window closed with glass, *c* the hollow which is filled with æsculine solution through a hole at the side which is closed with a screw plug with a minute perforation. The eye-lenses must be adjusted to the proper distance for distinct vision of the inner face of *a* before the eye-piece is inserted into the telescope.

(3) *On a spectrometer and universal goniometer adapted to the ordinary wants of a laboratory.* By Professor LIVEING.

The instrument exhibited to the Society was designed for use as a spectroscope, for the measurement of refractive indices and generally for all purposes in which angular measurements are required. Its chief features are first the steady support given to the telescopes by the peculiar form of the brackets which carry them. Each bracket has a vernier, and the telescopes can be brought up to one another as close as the diameters of the object-glasses will allow. Above the circle is a turn-table provided with a vernier which can be moved through the whole 360° without interfering with the telescopes, so that the angular motion of any

object upon it can be read while the telescopes are fixed. It can be used as a repeating circle if desired. There are adjustments for level of the telescopes and they can each be rotated in azimuth on their brackets when it is required to use them with a combination of prisms.

The instrument is provided with a prism with three polished faces giving refracting angles of 45°, 60° or 75° at pleasure and good definition in each position. The same prism can be used as a "half-prism" by placing its short face nearly perpendicular to the axis of the collimator, when the light will undergo internal reflexion at the next face and be dispersed at emergence from the third face. In this position of the prism the telescope and collimator are parallel and nearly in the same straight line, so that the instrument can be used as a direct vision spectroscope which is sometimes useful.

The instrument has been well constructed by A. Hilger of Stanhope Street, London, N.W., and is shewn in the photograph, Plate IX.

(4) *On some points in the structure and development of the Leaves of Pinus Silvestris L.* By T. H. CORRY, B.A., Caius College.

Beyond a few scattered notices[1] and figures of the adult leaf, fairly accurate but by no means satisfactory, the minute structure and development of the leaves of Pinus, though they present many features of peculiar interest, seem to have been hitherto passed over without almost any notice by investigators. F. Hildebrand it is true has contributed to the *Botanische Zeitung*[2] the results of a somewhat cursory comparative study of the appearances presented by the *adult* stomata in various members of the Coniferae, while Sachs has expressed his idea as to the method of origin of the so-called "intrusive ingrowths" in the cells of the cortical pallisade ground-tissue of Pinus. E. Strasburger[3] has studied completely the development of the stomata in Salisburia, Dammara, and Araucaria. Haberlandt also has endeavoured to explain the physiological function which the "intrusive ingrowths" exercise and the need for their occurrence. Further than this no account has been given.

I have been engaged at intervals for some time past in studying the features which these leaves present, but since a considerable time must necessarily elapse during the preparation of a series of

[1] C. E. Bertrand, "Anatomie Comparée des Tiges et des Feuilles chez les Gnetaceæ et les Coniferæ," *Ann. des Sci. Nat.*, Ser. v. Vol. xx. 1874, pp. 5—153, Pl. 1—12.

[2] "Der Bau der Coniferenspaltöffnungen und einige Bemerkungen über die Vertheilung derselben." *Bot. Zeit.* 1860, No. 17, pp. 149—152, Pl. IV.

[3] E. Strasburger, "Zur Entwicklungsgeschichte der Spaltöffnungen." Pringsheim's *Jahrbucher für Wissenschaftliche Botanik*, Vol. v. Pt. 3, 1867, pp. 297—342, Pls. XLI. XLII.

illustrative plates, and as I am aware that this subject is even now engaging the attention of others, it has seemed to me advisable to publish the following preliminary account of my investigations on the foliage leaves of the Scotch fir, Pinus silvestris, L. Before entering upon the points of interest in their development, a few remarks by way of prelude upon their position, shape, and external appearance will not be out of place. The leaves of this species are dimorphic; in other words the permanent elongated woody branches or terminal shoots of the year produce *no* foliage leaves, but each of them produces only a single deciduous chaffy and membranous scale, *i.e.* cataphyllary leaf. These scales consist only of leaf-sheaths without laminae, and in the axil of each a special abortive and dwarf branch or shoot arises which always remains very short, and this latter bears first a solitary hyaline scarious scaly leaf forming a membranous sheath for the inferior part of the branch, and then at the apex the foliage-leaves. Since the species I have worked with belongs to the sub-genus *Pinaster* only two green leaves are borne in a fascicle on each shoot; they are destitute of sheaths or stipules, they are sessile and their upper surfaces face each other. They are rather long (1—3 inches), narrow, rigid, and linear in shape and are best described as acerose, their point being acute. In transverse section they are nearly semicircular, *i.e.* convex beneath, and flat or very slightly channelled above. They are very persistent remaining on till the third year when they fall. The foliage leaves are girt at their bases by the scarious scales before mentioned. I now pass to the chief features in their development.

I. *Dermatogen.*

Development of the Stomata. The dermatogen cells form a single layer, in outline they are polyhedral and somewhat elongated, their long axes corresponding with that of the leaf; they are rather deeper than broad and their inner faces are slightly angular. They are bounded by perfectly flat walls. At numerous intervals on both surfaces of the leaf, but especially on the convex lower surface, a nearly cubical piece becomes separated and cut off at one end of an epidermal cell by a radial septum which runs in a plane transverse to the length of the leaf. This segment speedily grows in extent, and becomes large and conspicuous though it still retains its quadrangular form. Its protoplasmic contents stain very deeply and it possesses a large oval nucleus. Each of these cells constitutes the *mother-cell of a stoma.* So far the mode of development and preparatory division resembles very much that characteristic of Hyacinthus and Iris[1].

[1] Strasburger, *loc. cit.* pp. 300—304, Pl. xxxv. figs. 1—14.

Radial division now occurs in each of the four epidermal cells immediately adjoining the mother-cell of the stoma (one of them being the remainder of the original epidermal cell from which it was cut off), but without any regular sequence: the result of this is that the mother-cell is surrounded by a group or circle of four cells decussating in pairs, each containing a very large and evident nucleus and protoplasm which stains very deeply. This segmentation of complemental cells takes place invariably *before* the division of the mother-cell in Pinus and not *afterwards* as Strasburger found to be the case in *Salisburia adiantifolia* and *Araucaria imbricata.*

The nucleus and protoplasmic contents of the mother-cell may in the next stage be observed to have undergone division into two in the longitudinal plane of the leaf, the karyolytic figure formed in the change being frequently observable. This division is followed immediately by the formation of a radial wall of cellulose in the plane of section dividing the two parts. Thus the two *guard-cells* are formed. When the leaf is examined in transverse section the hypodermal cells, which are also elongated in the direction of the length of the leaf, and which elsewhere form a layer two cells thick around the periphery of the leaf, are found to be only a single cell deep under each stoma-mother-cell. By this feature as well as by their large size the position of the mother-cells of the stomata may be easily recognised in this view at an earlier period. Two cubical cells of the hypoderma appear in transverse section to underlie the mother-cell of the stoma, their line of union corresponding exactly to that of the divisions between the guard-cells so that the latter appears to be a direct continuation of the former. The guard-cells now elongate slightly in a radial direction, and since, owing to the mother-cell from which they were produced being slightly angular internally, the pressure which they exert in so doing is greatest at that point, the underlying hypoderm cells become in accordance pyriform in section and the line of union between them becomes rapidly reduced to a minimum. Very soon these cells separate from each other laterally, leaving a small intercellular space between them into which the inner and apposed ends of the guard-cells tend to project. Owing to this growth downwards of the guard-cells it comes about that their outer external ends are now slightly depressed laterally from their point of connection with each other below the surface of the four complemental cells of the group (two of which are seen in section), and of the other epidermal cells generally.

In the next stage the lateral divergence of the two hypodermal cells below the guard-cells, the downward projection of the internal ends of the guard-cells into the increasing dimensions of the intercellular space so formed, and the consequent depression of the guard-cells, all become still more marked. These also form the

prominent points in the several stages succeeding this, until at length the guard cells appear to lie as much in the hypodermal row as in the row from which they were originally derived, *i.e.* between the epidermal and the hypodermal cells, and are only separated by a slight space from the peripheral layer of the cortical ground tissue. The two hypodermal cells which they have displaced now lie partly beneath and partly at the sides of the guard-cells. The latter are now so much depressed below the surface that they are separated from it by an elongated oblong funnel, pit, depression or fossa forming an ante-chamber which is bounded laterally by the four complemental cells of the group under the inferior borders of which they are let in. These complemental or accessory cells have in their growth and development kept pace with the other cells of the epidermis as they do in Salisburia and consequently are of large size: their upper free extremities have also converged slightly towards each other over the funnel (since the pressure which previously existed has been relieved in that direction), and have become somewhat rounded[1].

In longitudinal sections, and when viewed from above, the guard-cells have an elongated, crescentic form. The walls of the epidermal cells, which have assumed in the meantime a fine irregular wavy or sinuous outline, though still preserving their original polyhedral form, now become in great part cuticularized, and this is especially the case on those borders of the four complemental cells belonging to each group which are towards the ante-chamber and are excessively thickened; while those of the hypodermal cells become sclerenchymatous. Thickening, accompanied by cuticularization of the wall separating the two guard-cells, also takes place, but unequally, the thickening being greatest in the middle and more pronounced towards the internal than towards the external side. In the next stage the two hypodermal cells have come to lie entirely laterally, the wall between the two guard-cells then splits down the middle, and the guard-cells separate from below upwards diverging as they do so but still remain attached at their outer ends. They alone contain chlorophyll grains, and are thus well marked off from the other epidermal cells. The last stage of all consists in the complete separation of the guard-cells from each other to form the cleft and the adult state of the stoma (which alone has hitherto been figured)

[1] The funnel is thus slightly contracted in the upper part by the projection of the epidermal cells, expanded in its middle part and again contracted slightly at the base above the guard-cells. Its depth is not very great as compared with those in the leaves of other Conifers, since the hypoderma is only at most two cells deep, and as Hildebrand has shown, a distinct and definite relation exists between the depth of this layer and that of the funnel above the guard-cells.

The upper walls of these four complemental cells moreover lie on a perfect level with the general surface of the epidermis, and are not in any way either depressed beneath it or elevated above it. Thus the stomata belong to Hildebrand's second type (*loc. cit.*), which is the most frequent one in the Coniferæ.

is attained. The guard-cells now lie almost horizontally; though derived from the epidermal layer they lie *completely* at the level of the hypoderma, but they may easily be distinguished in that they alone of all the epidermal and hypodermal cells contain chlorophyll grains. A large intercellular space usually underlies each stoma; this is formed by the wall of a cell belonging to the cortical ground tissue which immediately underlies the stoma, being caught inwards towards the centre of the leaf owing to the presence of one or several of the so-called "intrusive ingrowths," to be immediately referred to, in it in that position. The arrangement of the stomata is invariably in linear longitudinal rows, and the long axes of the clefts lie in the same plane as the long axis of the leaf while the guard-cells lie right and left of them: since they occur on both sides of the leaf alike they are found in situations which are exposed to light as well as those which are sheltered from it, and are most numerous in the former. The type of formation exhibited by the stomata of Pinus belongs to the second great type of Strasburger (*loc. cit.*) where several epidermal cells take some part in the formation of a stoma, and to the second division of that type, viz. that in which the divisions to form complemental cells affect *all* the cells immediately surrounding the stomata-mother-cells. It is most nearly related to the type of *Salisburia adiantifolia* and then to *Cycas revoluta* and *Araucaria imbricata.*

Except in the points just noticed the epidermal cells do not present any marked peculiarity, their external walls are cuticularized to an extremely large extent, and their walls are much thickened and exhibit stratification and well-marked canals running outward from the lumen, which is consequently very much reduced and almost obliterated. The line of distinction between the portion of the wall which is merely thickened but not chemically changed and that which is so altered is easily rendered evident on application of Chlor. Zinc. Iod. (Schultze's) solution.

At each angle of the leaf in the transverse section an epidermal cell which is very much larger than the rest projects, the external convex wall of which is especially thickened.

II. *Periblem.*

**Ground Tissue or Mesophyll.*

A. Hypoderma. The cells of this when mature are very thick-walled so as to be sclerenchymatous, exhibiting well-marked middle lamellæ, stratification, and canals running from the very much reduced lumen to the exterior; they are not however coloured. They are elongated in a direction parallel to the length of the leaf and prosenchymatously arranged, forming fibres which contribute to the greater firmness and elasticity of the epidermis which they

immediately underlie. Except in the regions where these cells underlie the mother-cells of the stomata the layer is usually two cells thick and may even attain a depth of three cells at the ends. They long retain their protoplasmic contents and nuclei but never contain chlorophyll grains. By some *e.g.* Hildebrand, Haberlandt &c. they have been called "liber-cells" or "sub-epidermal bast cells," since in general characters they somewhat resemble bast-fibres but, as MacNab[1] has already pointed out, they have no relation whatever to the fibrovascular bundle, of the phloem of which the true liber-fibres when they occur always form an integral part, being simply sub-epidermal in position. Similar cells also occur in Lycopodium. The hypoderma in the *mature* leaf is conspicuous and forms a continuous band except beneath the stomata.

B. Cortical. The cells are arranged in transverse section in several (2—3) concentric rows extending round the whole periphery of the leaf and in most cases contain two or more nuclei: in the earlier stages they are somewhat polygonal in transverse section, flattened and columnar in shape, and of large size. Their nuclei lie centrally or nearly so, and are connected with a protoplasmic sac (primordial utricle), which closely lines the walls on all sides[2], by means of very numerous fine and delicate filaments or strands of protoplasm passing across the cell vacuole so that the nuclei present a stellate form[3].

The cells are united together parenchymatously, but are closely packed in such a manner as to form a most perfect palisade, no inter-cellular spaces being at this time observable. At a somewhat later period one or several of these protoplasmic strands may become converted into cellulose, and thus by this means each of the nuclei is firmly attached to the wall. The change is easily demonstrable by the action of staining reagents, and for this purpose I have used aniline colours[4] and also dilute tincture of Iodine followed by Chlor. Zinc. Iod. (Schultze's) solution; in the earlier cases all the strands invariably stain after treatment with Hoffmann's Blue or Aniline Blue, and turn Brown with Chlor. Zinc. Iod., while in the later stages those filaments which have become converted into cellulose refuse to become stained with the two

[1] Histological Notes, IV. "The thickened cells in the leaves of Pinus," *Jour. Bot.*, Vol. X. 1872, p. 33. Also *Trans. Bot. Soc. Edin.*, Vol. XI. 1872, pp. 296, 297.

[2] I was enabled to demonstrate the presence of this latter by plasmolysing fresh material and also by staining thin sections of absolute alcohol material with Hoffmann's Blue.

[3] Gardiner has recently shown that the occurrence of similar filaments connecting its ends to the primordial utricle causes the elongated shape of the nucleus in Spirogyra, an observation which I have been able fully to confirm.

[4] The method I have employed is to dissolve the dye in Absolute Alcohol diluted to 50 per cent. strength; stain the sections in this, then wash with water and mount in strong pure Glycerine.

aniline colours just mentioned, yet these same strands on treatment with Methylene Blue or the Chlor. Zinc. Iod. method stain in the manner characteristic of undoubted cellulose walls. So far there is not the slightest appearance of any infolding or intrusive ingrowth of the cell wall having taken place, for its outline remains perfectly straight, uniform, and is very little thickened if at all. Those filaments which have not become chemically altered remain in their protoplasmic condition stretching from the nucleus to the primordial utricle lining the cell-wall. When an older stage is examined each cell may be observed to have increased in size over the entire periphery, with the exception of the points of insertion of the cellulose or protoplasmic filaments which firmly attach it to the nucleus: at these places it is held in and prevented from doing so. The cell-wall is moreover observed to have become thicker. As a consequence of this tension at certain points the outline of the cell becomes irregular and presents depressions or bays along its walls. The cell still continues to increase in size while the points before mentioned are still held in, and soon slight *apparent* infoldings or involutions (of the cellulose wall, the double nature of which is obvious), proceeding towards the interior become observable in the cell cavity as shallow depressions which become elongated and tubular, and give the cell a very peculiar and characteristic aspect. The two limbs or halves of each of these infoldings rapidly come to be very closely approximated, except sometimes at their outer and inner ends, and form apparently a single ingrowth which is slightly thickened and pyriform in shape. The space between the two limbs is now merely a line, while the extremity which projects into the lumen is thickened and fillet-shaped. The enlargement of the cell goes on, and the result is that many of the protoplasmic filaments break away from their attachment to the apparent infoldings of the wall and are probably withdrawn into the immediate neighbourhood of the nucleus. The majority of the apparent infoldings or ingrowths[1] therefore remain small and abortive, but one or several remain to which the nucleus is attached either by protoplasm or by the intermediation of a strand or strands of cellulose (formed by conversion of the pre-existing protoplasmic strands as already described), or else directly.

The nucleus in many cases exhibits a tendency to become attached directly to one of the infoldings of one wall, this is due to the filament of protoplasm which connected it being shortened and the pseudo-infolding being in this way drawn towards it until the two meet: the nucleus still however retains its connection by cellulose or protoplasmic filaments with other walls. In some cases a band of cellulose is formed by conversion of the protoplasm round

[1] They all give the characteristic cellulose reactions with Methylene Blue and the Chlor. Zinc Iod. method.

the nucleus, which unites two of the apparent cellulose infoldings, and slings the nucleus firmly to one wall. Where the nuclei in two neighbouring cells are connected in the early stages by filaments of protoplasm to the same point of the common partition-wall between them it comes to pass that in the process of enlargement of the cells, when the walls are become thickened, this wall being strained in two opposite and contrary directions splits along its centre and there results at a slightly later period two opposite pseudo-ingrowths of cellulose, often leaving an intercellular space between them[1]. To the irregular form which the cells assume in consequence of their unequal enlargement, owing to parts being held in, the intercellular spaces which are met with in the tissue of the adult leaf are due; for in the early stages as has been already mentioned they are absent. The nuclei in these cells are never free, and in this feature they present a marked contrast to what occurs in the case of the colourless medullary parenchyma where the protoplasm disappears early and the large circular nuclei are nearly always found lying freely in the cell cavity. The only explanation which has been hitherto advanced as to the nature of the so-called "infoldings" which form such a characteristic feature of the adult leaf and have long been known to exist there is that given by Prof. Sachs, who considers that "on the partial splitting of the partition wall a local growth of one or both of the two lamellae (or of one only) takes place so that a fold arises which intrudes into the cell cavity[2]." This view is based partly upon the appearance presented in section by the cells of the adult leaf and partly upon the analogy of certain other cases there quoted; it is *not* however as I have shown supported by the results obtained from a study of the facts of development, and is contrary to the usual mode of growth and expansion of the cell. The so-called "intrusive folds or ingrowths" are not at all of this nature, for no intrusion or real invagination ever takes place; they simply and only occur at the points where the protoplasmic filaments of the nuclei in the original columnar cells were connected with the walls, and where in the subsequent enlargement of the cell-wall retardation of growth has occurred. How the *appearance of intrusion* comes about I have already explained. Some of the simplest and least complicated cases may always be seen in the walls of the

[1] I feel sure that the so-called "pit-like formation" figured by Sachs (*loc. cit.* fig. 60) is not of this nature, for I have never observed any appearance of pits of this kind, but is simply due to two abortive pseudo-ingrowths on a longitudinal radial wall, which having caused a splitting of the common partition, and the formation of two shallow bays, have then ceased to be connected with the nuclei, owing to the breaking away of the protoplasmic filaments which united them to it; and I have been able to observe every intermediate stage between these so-called "pit-like formations" and characteristic pseudo-ingrowths.

[2] *Text-Book of Botany*, 2nd English Edition, 1882, p. 74.

cells forming the subhypodermal layer of this cortical tissue, particularly in those which immediately underlie the stomata, for in these cases an opportunity for greater growth seems to be afforded than it does elsewhere, and the apparent ingrowths are formed on one side of the wall only, while in many cases the two limbs of the bay may still be seen to be separated. They are also well exemplified in the last layer of cortical cells, *i.e.* that which immediately abuts on the medullary parenchyma.

Pseudo-intrusive foldings such as those just described are not however confined to the leaves of Pinus although they were first discovered in this genus, for L. Kareltschikoff[1] observed their presence in the palisade tissue of the leaves of certain Grasses, viz. species of Bambusa (*stricta, tecta, verticillata, latifolia*), Arundinaria spathulifolia, and species of Elymus (*Canadensis, arenarius,* and *mollis*); while G. Haberlandt[2] has found them in the leaves of many plants belonging to the natural order Ranunculaceæ[3] and in Sambucus nigra! among Dicotyledons; in Alstrœmeria psittacina, Bambusa Simonii, and in several species of Calamagrostis (*Epigejos, stricta! Halleri* and *silvatica*) among Monocotyledons; in the leaves of all the species of Pinus among the Coniferæ; and in the fronds of several ferns belonging to the genera Adiantum (*Capillus-Veneris!*, and *trapeziforme!*) Aspidium (*aculeatum!* and *Sieboldii*) Lomaria (*gibba*) and Todea (*aspera*). I have also in addition seen them well developed in the curious leaves of the Grass *Pharus latifolius* and from a figure of Elsberg's[4], I believe they are present in the cells of the petals of the Flowering Flax, (*Nierembergia gracilis,* Hook). In many cases but not in all they appear to have a tolerably definite relation to the outer surface of the leaf, being arranged so as to be perpendicular to it. In some cases they are apparent only on the upper wall of the cell, in others on both upper and lower. All stages however exist between these cases and those *e.g.* Pinus, Elymus mollis, and the

[1] "Ueber die faltenförmigen Verdickungen in den zellen einiger Gramineen," *Bulletin de la société imperiale des naturalistes de Moscou*, Vol. XLI. 1868, p. 180.

[2] "Ueber eine eigenthümliche Modification des Pallisadengewebes," *Oesterreichische Botanische Zeitschrift*. Wien, Vol. XXX. No. 10, pp. 305, 306, Oct. 1880. (Abstracted in *Jour. Roy. Mic. Soc.*, Ser. II. Vol. I. Pt. 1, 1881, pp. 70, 71.) Also "Vergleichende Anatomie des assimilatorischen Gewebesystems der Pflanzen," in Pringsheim's *Jahrbücher für wissenschaftliche Botanik*, 1881, Part XIII. Sect. i. pp. 74—188, Plates iii.—viii.

[3] Viz., Trollius Europæus!; Caltha palustris!; Aconitum Napellus! and A. dissectum; Pæonia corallina!, and P. tenuifolia; Clematis integrifolia!, and C. recta; Anemone silvestris, A. japonica!, and A. memerosa!. In those species marked with a mark of admiration I have verified their occurrence, and also in Anemone appenina. They do not occur in the following species: Ranunculus Ficaria, R. repens, and R. acris; Helleborus niger, H. fœtidus, and H. viridis; Anemone pratensis; Eranthis hiemalis; Aquilegia vulgaris, and A. calafornica-hybrida; Delphinum dissectum, D. grandiflorum, D. fissum, and D. truncatum.

[4] "Plant cells and Living Matter," *Quart. Jour. Mic. Sci.*, N.S. No. LXXXIX Jan. 1883, pp. 87—98, fig. 4.

species of Adiantum and Aspidium, where they are arranged quite irregularly, occurring on *all* the walls and having *no* definite relation to the surface of the leaf: their direction is therefore by no means constant[1].

Their presence is by no means uniform even in the same genus, for they are absent from the leaves of *Anemone silvestris*, though present, as has been mentioned, in other species; and they are wanting in *Alstrœmeria bicolor* and *A. aureum* though present in *A. psittacina.* They are further not absolutely confined to palisade tissue although they are of very characteristic occurrence there, for in *Bambusa Simonii*, Haberlandt found them prevalent in the cells of the sub-epidermal layer of the under surface of the leaf as well as in the two rows of palisade cells on the upper surface. Very frequently in the same leaf and even in the same section ordinary palisade cells occur at intervals among palisade cells characterized by the presence of these pseudo-ingrowths *e.g.* in many Ranunculaceæ, in *Elymus canadensis*, and in *Todea aspera.* I have examined the development of these pseudo-ingrowths in the leaves of several of the species in which they were discovered by Haberlandt and Kareltschikoff, and have in all cases found that they have a mode of origin similar to that which I have already described for those of Pinus. In all cases they bear a definite relation to the nucleus, which is placed in close proximity to their internal ends, and with which they are connected either by protoplasmic or cellulose strands. The chlorophyll corpuscles are always very numerous in the protoplasm (primordial utricle) lying along both sides of each pseudo-ingrowth just as they are on the lateral walls of the cell.

Haberlandt in the most recent of his two papers draws some curious, interesting, and somewhat startling conclusions as to the physiological significance of the pseudo-ingrowths, and the probable conditions which determined their origin. He considers that the cells which possess them furnish the clue to the main function of

[1] I have been unable to confirm Haberlandt's observations as to the relative direction of these ingrowths in *Pinus silvestris*, the species which we both alike have examined. His account is as follows: "In the innermost palisade cells nearest the colourless medullary tissue the folds show no definite arrangement in relation to the surface of the leaf; but the nearer the cells lie to the periphery of the leaf, the more the folds endeavour to assume a position perpendicular to the surface even when, as often happens, they have thereby to make a sharp angle with the wall of the cell on which they occur. The external cells lying under the sub-epidermal bast layer (hypoderma) show folds perpendicular to the upper surface of the leaf. Two folds are at most present in each cell which project opposite one another into the lumen of the cell, and so produce an H shaped cavity." I have found, on the contrary, as many as five or more pseudo-ingrowths, even in the sub-hypodermal cells, which are very frequently *not* H shaped; some of these folds are parallel to each other while some spring from the lateral walls, but I have never been able to observe any definite relation such as that just described, although I have carefully looked for it.

palisade tissue generally, and argues that since one and the same leaf contains palisade cells both with and without pseudo-ingrowths the physiological functions performed by the two sets of cells is the same. He believes that the cells in which pseudo-ingrowths occur irregularly are simpler than those in which they have a tolerably definite relation to the periphery of the leaf, and simpler also than the ordinary palisade cells which are destitute of any such infoldings! Since a fold projecting into the lumen of a cell must of necessity increase the inner surface of the cell-wall, and this is correlated with the fact that the chlorophyll corpuscles always lie in close contiguity with the walls of the cell, he argues that the function of such an infolding must be to increase the efficiency of the cell as an assimilating organ by affording space for more chlorophyll corpuscles within a given area. As already mentioned, the protoplasm (primordial utricle) close to the pseudo-infoldings is always beset with chlorophyll corpuscles, so that this argument is fairly borne out by the facts. He however goes still further. Excluding any direct causal relation of the infoldings with illumination by light on account of their irregular direction, he considers any idea that they might serve to afford increased facility for the transmission of air into the interior of the leaf refuted by the fact that in *Trollius europœus*, *Bambusa Simonii* and, according to his researches, frequently in Pinus no air-space exists between the limbs of the closely apposed folds, which ought of necessity to be invariably present on this view of their function. Such air-spaces however he admits *do* exist in the infoldings of *Sambucus nigra*, *Pæonia tenuifolia*, and *Anemone silvestris*, and there they are sometimes moderately broad. He concludes that the physiological principle of increase of surface conditions the presence of these pseudo-infoldings, which he evidently considers to be a definitely designed, and not as they are a comparatively accidental feature, and that their number and size are determined also by the same cause; although at the same time he considers his explanation unavailable for those in the leaves of Coniferæ. He would also explain the size, number, and occurrence of the actual radial longitudinal partition-walls between the palisade cells by the same method and compares them simply to completed infoldings and the pseudo-ingrowths to incomplete partition walls!

Conclusions such as these based upon an entire misconception of the real mode of origin and development of the apparent ingrowths and of their morphological nature are of necessity erroneous. From the account I have given it will be at once evident that *no* correlation as to their mode of formation such as Haberlandt would institute between the radial longitudinal partition-walls and the pseudo-infoldings is at all possible. The

first are formed by a very definite act of cell-division, the latter merely as the result of unequal growth; and, though the fact that the chlorophyll corpuscles are closely applied to both is undoubted, yet this feature is probably rather a secondary adaptation to environment determined by the presence of the folds, and not the main moving cause in their production, for if it were we should expect them to be constant, and not as they are, very limited in their occurrence. The idea that these pseudo-ingrowths serve as air channels to facilitate the transpiration of watery vapour as well as affording an increase of surface, is I think from the facts of development very much more probable than Haberlandt seems to regard it, since I have invariably found them in Pinus and in every other case I have examined, even in *Trollius europæus, at least at their first origin,* to exhibit a small air-space. In connection moreover with the recent researches of Boussingault on the absorption and metabolism of Carbon Dioxide gas by leaves they would also play no unimportant part in distributing the air which has entered the leaf through the walls of the epidermal cells. They perform at all events a very obvious and noteworthy function in forming the intercellular spaces beneath the stomata in Pinus, and in producing air channels between the cells forming the several rows of the palisade tissue; but whatever functions they perform are mainly the result of adaptation on the part of the leaf to derive the greatest possible advantages from their presence when they first occurred, and the leaf having become so adapted in a favourable manner their existence was perpetuated. The results which I have obtained as to the conditions under which the pseudo-ingrowths are developed seem to me to have considerable general importance as pointing out the existence of an intimate connection between the nucleus and the cell-wall such as Strasburger and Schmitz have shown to be present between the protoplasm and the latter.

The cells of the cortical ground tissue contain numerous chlorophyll grains, and after treatment with absolute alcohol to dissolve out the pigment, the protoplasmic basis of these then exhibits the reticulated organized appearance first described and figured by Pringsheim[1]. No spongy tissue is present.

The resin-ducts—passages—or canals. These run parallel to the longitudinal axis of the leaf and their number is not very constant; a pair of large ducts are however constantly present just below the angles of the leaf where the upper and lower leaf surfaces join one another. These Kreuz, who has made a careful study of the comparative structure of the resin-ducts in the Coniferæ

[1] "Ueber Lichtwirkung und Chlorophyllfunction in der Pflanze," Pringsheim's *Jahrbücher für wissenschaftliche Botanik*, Vol. XII. 1881.

generally, terms the "constant resin-canals." They follow the course of the fibrovascular bundle from the stem into the leaf and join in the stem, the main cortical resin-canals running longitudinally from below upwards parallel to the fibrovascular bundles.

Beside these numerous other smaller resin-passages are to be met with in the leaf, the number of which is very inconstant but often there are nine or ten of them, the three most frequently present being an unpaired median, one in the centre of the flat upper surface, and one on each side of the lower surface equidistant from those at the ends and also from the median ventral line of the leaf. These inconstant resin-ducts do not all appear simultaneously but have a definite order of sequence, those on the upper surface always appearing last. They disappear also towards the apex of the leaf in the same order in which they appear, and at the extreme point only the two constant ones are to be found. Their mode of formation is as follows:

At a very early stage indeed in the history of the leaf when all the cells are still in a meristematic condition and long before the first trace of the formation of the guard-cells of the stomata can be distinguished the origin of the resin-ducts may be observed in the palisade tissue.

In the position where a resin-duct is about to be formed a single cell may in transverse section be seen lying in the midst of the large chlorophyll-containing cells of the cortical parenchyma, but differing from these latter slightly in size, form, and markedly in being filled with very granular protoplasm which is destitute of chlorophyll grains and is therefore colourless. This is the *mother-cell of the resin-duct.*

This mother-cell divides first into two semicircular segments by means of a longitudinal wall, and then each of the segments in turn divides into two by a wall in the same direction as the first wall but in a plane perpendicular to it. Thus a group of four quadrant shaped *daughter-cells* is produced, each containing protoplasm and a nucleus. These four cells then enlarge, after which they tend to round off slightly, and, in consequence of this, certain of the partition-walls split and separate from each other at the common point of union or convergence of the cells. This separation takes place very gradually and slightly at first, and its result is a small intercellular space which is the *cavity* or *canal* of the resin-passage. Before this separation takes place however, some at least, and often all of the four daughter-cells begin to divide by walls in a direction tangential to their common point of convergence, and so a circle of cells immediately exterior to those which surround the canal of the resin passage, and form the resin-wall is produced. Neither these nor the cells of the resin-wall ever contain chlorophyll grains at any stage. Owing to the enlargement of the cells of the

resin-wall at the time when they separate, those cells which compose the circle exterior to them are pressed upon from within and each usually divides tangentially by a wall into two segments. The cells of the resin-wall themselves also divide tangentially and their more external segments are added to the outer circle. Each cell of the resin-wall now becomes divided in turn by a radial wall, so that six and then eight cells result, which then round themselves off so that the resin passage becomes larger. Some or all of these may divide again radially so that there are sometimes twelve, fourteen, or sixteen cells in the completed resin-wall. The cells of the resin-wall form thus a sort of membrane or secreting epithelium only one cell deep around the cavity of the resin passage, which latter has a schizogenous mode of origin. While the cells of the resin-wall are being divided radially, divisions in a similar plane also occur in the numerous cells lying exterior to them which have also been derived from the primitive mother-cell: these divisions enable the cells of the outer circle to keep pace with the increasing dimensions of the resin-wall within. The number of rows of cells in the external circle bears however a strict relation to the ultimate number of cells forming the resin-wall: thus if the four daughter-cells which constitute the primitive resin-wall divide only a small number of times, *i.e.* so as to form only six or eight cells in all (a not unfrequent occurrence), then radial division of the cells of the outer circle also ceases early and they begin to increase by tangential walls instead, forming not one but two or three circles of cells. If on the other hand the cells of the primitive resin-wall divide radially a large number of times so that there are as many as fourteen or sixteen cells in all, then the cells of the outer circle have little time to divide tangentially and hence form only a single row. In the former case the cell-walls of the cells forming the outer circle become greatly thickened and sclerenchymatous, so that their cell cavities are rendered very small or may disappear completely, their cell contents disappear early, and they form a firm protective sheath or investment for the resin passage and the resin-wall, giving rigidity to them in order that the cavity of the passage may not be closed up by pressure from the midrib or other prominent portions of the leaf. They are not however in any way hypodermal cells as Mons. C. E. Bertrand (*loc. cit.*) and Dr Maxwell T. Masters seem to regard them[1]. In the latter case however the walls of the cells of the outer circle never have time to become greatly thickened before the completion of the resin-duct. So much can be made out by means of transverse sections. Since however to each cell of the group of four daughter-cells a longitudinal row of cells lying one over the other corresponds,

[1] Vide "On the Relations between Morphology and Physiology in the Leaves of certain Conifers," *Jour. Linn Soc. Bot.*, Vol. XVII. No. 105, pp. 547—552, March 1880.

and between the successive groups separation takes place in a corresponding way, the result is of course a series of intercellular spaces continuous with one another and forming a regular canal of very considerable dimensions. The resin-wall of this canal is formed of oblong cells whose longest diameter runs parallel to the long axis of the leaf. These resin-wall cells are specially modified for the purpose of actively secreting or excreting a resin dissolved in turpentine so as to form a semi-fluid balsam, and this product is poured into and densely fills the cavity of the resin passage which acts as a reservoir for it. The resin secreting cells are easily distinguished by treatment with Tincture of Alcanin (Alkanet) when they alone are deeply stained. My observations afford complete confirmation of the accuracy of those of Kreuz[1] as to the mode of derivation of the thickened sclerenchymatous protecting wall surrounding each of the resin-ducts from the primitive mother-cell of the resin-duct, and are opposed to those of Frank[2] who states that when the four daughter-cells have increased by division to eight the parenchymatous cells in the row outside them lose their chlorophyll and become sclerenchymatous. They are also opposed to those of Meyen[3] who states that the cells which occur behind (exterior to) those which at first line the passage, step at a later period into this inner circle and serve to increase the number of cells there. Like Frank and Kreuz I have traced the origin of the ducts each to a single mother-cell in transverse section and I therefore differ from N. Müller[4] who states that they arise from the separation of four primitive cells.

C. *Medullary.* The cells of this tissue remain for the most part thin-walled, they are flattened and columnar with their long axis perpendicular to the periphery of the leaf, except in the case of the outermost layer, in which the cells are short and cubical their long axes being parallel to the periphery. This layer forms a sheath for the fibrovascular bundle, and the walls of the cells are thickened, especially on the side towards the palisade tissue. They are united together without intercellular spaces, and their protoplasmic contents, with the exception of a parietal layer lining the wall, disappear at a very early period in the leaf's history, leaving a single nucleus of very large size and oval or round form usually lying freely in the cell cavity and not in any way connected with the walls.

These nuclei when treated with staining re-agents, *e.g.* Saffranin, Haematoxylin, Osmic acid, or Methyl green, exhibit the character-

[1] "Beiträge zur Entwicklungsgeschichte der Harzgänge einiger Coniferen," *Sitzb. der k. Akad. der Wissensch.* Wien, LXXVI. Oct. 1877.

[2] *Beiträge zur Pflanzenphysiologie*, 118—123.

[3] *Phytomie*, p. 191; *über Secretionsorgane*, p. 18.

[4] Pringsheim's *Jahrbücher*, Vol. v. p. 399.

istic reticular structure described by Schmitz[1]. The cells are readily distinguished from those of the more peripheral cortical tissue by the complete absence of chlorophyll corpuscles in them so that they appear colourless, as well as by the early disappearance of most of their protoplasm, and the characters of their nuclei. At a comparatively late stage a development of bordered pits occurs both on their radial-longitudinal, as well as on their tangential and transverse walls; these pits may be seen both in surface view and in transverse and longitudinal section, hence this tissue has been called "areolar tissue." They appear, (except on the cells of the sheathing-layer immediately adjacent to the palisade tissue where they are absent,) at a late period, being first prominent when the cell-walls separating the guard-cells of the stomata have become thickened but have not yet split. The mode of their development has been well described by Sachs (*loc. cit.* p. 23), and was first accurately recognized by Hermann Schacht[2]. Towards the base of the leaf this medullary tissue disappears. Below and also to a certain extent above the fibrovascular bundle are a few thick-walled sclerenchymatous cells which MacNab and other systematists of the group, term "liber-cells" though they belong to the ground-tissue.

** *The Fibrovascular bundle.*

This is single, but divides immediately on entering the leaf into two well-marked branches which are collateral in arrangement, very closely approximated and separated by a primary medullary ray; they have a very simple structure and end blindly. As regards their histological elements, the *xylem* which lies towards the upper surface consists only of a few spiral and annular vessels; and the *phloëm*, which is directed towards the lower and outer surface, consists of bast parenchyma and sieve-tubes, *i.e.* of soft bast only. No bast-fibres are present, and tracheides with bordered pits are completely absent from the xylem[3], hence these are probably not present in the primary wood of the stem formed from the procambium. No resin passages, as Van Tieghem has already remarked[4], are present in the portions of the fibrovascular bundles lying in the leaf though they are present in the parts of these same primary bundles which lie in the stem.

[1] *Sitzber. d. niederrhein. Ges.* in Bonn, 1880.
[2] *De maculis in plantarum vasis*, &c., Bonn, 1860.
[3] In this last I differ from Bertrand, who (*loc. cit.*) states they are present.
[4] "Mémoire sur les canaux Sécréteurs des Plantes," *Ann. des Sci. Nat.*, Series v. Vol. xvi. 1872.

Adult Appearance.

Since MacNab[1] has recently laid stress upon the features exhibited by the leaves in transverse section as a guide for the determination of species and genera, a brief *résumé* of the characters presented by those of P. silvestris is appended.

Leaf about twice as broad as it is thick, upper surface flat, lower convex. Epidermis possesses stomata in rows on both surfaces though they are most frequent in the convex surface.

Hypoderma conspicuous, forming a single or double layer around the whole periphery of the leaf, broken only immediately below the stomata.

Palisade tissue presents no apparent differences on the upper and under surfaces. All the cells closely packed, being 2—3 rows deep, forming a continuous zone surrounding the medullary tissue; there is no spongy tissue. The intercellular spaces are only due to the formation of pseudo-intrusive ingrowths, and their number is not great in proportion to the leaf. These cells contain chlorophyll.

Resin Canals, two large and constantly present, viz. one at each angle of the leaf near the margin, not far from the epidermis; one smaller unpaired median one usually present in the centre of the upper surface.

Medullary parenchyma, several layers of cells closely packed without chlorophyll and with conspicuous bordered pits; outer layer of short cubical cells, forming well-marked sheath to the fibrovascular bundle.

Fibrovascular bundle, single with two distinct branches, separated by a certain amount of medullary parenchyma forming a primary medullary ray, usually a few greatly-thickened sclerenchymatous cells above and below the bundle. No bast fibres.

May 14, 1883.

Mr Glaisher, President, in the chair.

At the Meeting of the Cambridge Philosophical Society held on Monday, May 14, Mr F. Darwin, M.A., Trinity College, was ballotted for and duly elected a fellow of the Society.

[1] "A Revision of the Species of Abies," *Proceedings of the Royal Irish Academy*, Ser. II. Vol. II. *Science*, pp. 673—704.

The following communications were made to the Society:—

(1) *On the highest wave of uniform propagation.* Preliminary notice. By G. G. STOKES, M.A., F.R.S., Lucasian Professor of Mathematics in the University of Cambridge.

There is one particular case of possible wave motion, applicable to a fluid of practically infinite depth, in which all the circumstances of the motion admit of being expressed mathematically in finite terms, the necessary equations being satisfied exactly, and not approximately only; while the general expressions contain an arbitrary constant permitting of making the amplitude any whatsoever up to the extreme limit of cycloidal waves, coming to cusps at the crests. This possible solution of the equations was given first by Gerstner, near the beginning of the present century. The motion however to which it relates is not of the irrotational class, and could not therefore be excited in a fluid previously at rest by forces applied to the surface; nor could it be propagated into still water from a disturbance at first at a distance. In fact, the conditions requisite for its existence are of a highly artificial character; so that the chief interest of the solution is one arising from the imperfection of our mathematics, which makes it desirable to discuss a case of possible motion, however artificial the conditions may be, in which everything relating to the motion can be pretty simply expressed in finite terms.

There can be no question however that it is the irrotational class of possible wave motions which possesses the greatest, almost the only, intrinsic interest; since it is this kind alone which can be excited in a fluid previously at rest by means of forces applied to the surface, such for example as the unequal pressure of the wind on the surface, or propagated into previously still water from a distance.

In a paper read before the Society in 1847, and published in the transactions, I have investigated the motion of oscillatory waves in which the motion is not very small by the method of successive substitutions, proceeding to the second order in the case of an arbitrary depth, and to the third order in the simpler case in which the depth is infinite. In the latter case the terms of the third order were found to be very small even in the case of waves of very considerable magnitude. The series converge less rapidly when the depth is finite; and when the length is very great compared with the depth of the fluid the convergence becomes so slow that the method practically fails, and is not therefore applicable to solitary waves.

The circumstances of the motion of solitary waves of consider-

able height have been investigated by M. Boussinesq[1] and Lord Rayleigh[2].

The evidence of the existence of a type of oscillatory irrotational waves which are uniformly propagated is derived from the nature of the process of approximation, which is one of a systematic character that can certainly be carried on to as many orders as we please, all the conditions of the problem being satisfied to the order to which we step by step advance. If therefore the series that we are working with be convergent, there can be no question of the possible existence of uniformly propagated waves. But for a given depth and given wave length there remains in our series a disposable constant on which the height depends, and on the value of which the degree of convergency depends. By taking this constant small *enough* the series will be convergent; though *what* the limit may be that separates convergency from divergency, the process of expansion does not show.

It seems to me pretty certain that the series will remain convergent until a singular point appears at the boundary of the fluid. Some years ago I was led by simple considerations to the conclusion that the occurrence of a singular point in the profile at which two branches meet at a finite angle (or as it might conceivably have been touch, forming a cusp) entailed as a consequence the existence of two tangents inclined at angles of $\pm 30^\circ$ to the horizon; so that the ridges of the waves came to wedges of 120°. In a supplement to my former paper[3] lately published I have conducted the approximation in a different manner, which is more convenient for proceeding to a high order. In this latter method the coordinates x, y are expressed in terms of the velocity potential ϕ and steam-line function ψ, instead of ϕ and ψ in terms of x and y. The approximation is carried to the fifth order for deep water, and to the third when the depth is finite. Still even in this method the labour of the approximation rapidly increases with the order, so that the result of working out a great number of terms would not repay the labour; and expansion by series is hardly applicable to the determination of the circumstances of the highest possible wave. When a series whose general term contains a power or other function of some parameter a is convergent when a lies below a certain critical value, and divergent when it lies above, it may be convergent when a has the critical value, but if so its convergence is very slow. If we allow that the highest possible wave comes to a ridge of 120°, that, combined with our knowledge of the form of waves of very considerable height, would enable us to draw very approximately the

[1] *Comptes Rendus*, Tom. LXXII. (1871) p. 755.
[2] *Philosophical Magazine*, Vol. I. (1876), p. 257.
[3] *Mathematical and Physical Papers*, Vol. I. p. 314.

theoretical outline of the highest possible wave. But it is tantalizing to get thus near it and not to be able to complete the solution.

The expansion in series would be of little avail for the reason I have mentioned; but it occurred to me that some method of trial and error might succeed. I have devised one which promises so well that a notice of the *method* may not be without interest to the Society, though I am not at present in a condition to present the *result*, except it were in a rough way, not having completed the numerical calculations required, nor even begun them in the second, and that the more interesting, of the two cases to which the method applies. I have employed the series contained in the supplement before referred to, not however directly, for the purpose of numerical calculation, but merely as stepping-stones enabling me to effect a certain analytical transformation in which the use of series is got rid of.

The method is not confined to the case of the highest possible wave, but may also be used for lower waves, though unless the wave is near the maximum it is better to have recourse to the series. In any case of uniform propagation, we may readily reduce the motion to a case of steady motion, and when that is done the velocity of a particle at the surface will be the same as that of a particle sliding along a smooth curve corresponding to the outline of the wave, and will accordingly be that due to the depth below a fixed straight line, which for the sake of a name may be called the *datum line*. In the case of the highest wave, since a particle at the vertex of a wedge must be momentarily at rest, the datum line will pass through the crest; in other cases its height above the wave must be assumed for trial as well as the outline of the wave.

The trial outline (and the trial datum line in the case of a wave short of the highest) being known, the velocity at any point of the surface is known, and therefore by an ordinary integration or by a quadrature the velocity-potential at the surface is known. Hence ϕ being known in terms of x, x is known in terms of ϕ. But the co-ordinates of points in the surface are given in terms of ϕ by equations (23), (24) of the supplement referred to on putting ψ, the parameter of a stream line, $=0$. These equations have been simplified by choosing the units of space and time such as to make a wave length, and also the change of ϕ in passing from one wave to the next, each equal to 2π, and k is the value of $-\psi$ at the bottom. The equations then become

$$x = -\phi + \Sigma A_n (e^{nk} + e^{-nk}) \sin n\phi \quad\ldots\ldots(1).$$

$$y = \Sigma A_n (e^{nk} - e^{-nk}) \cos n\phi \quad\ldots\ldots(2).$$

The negative sign of ϕ in the first of these equations arises

from choosing the direction of x positive as that of the propagation of the waves in the first instance, and therefore the direction of x negative as that of the superposed velocity by which the motion is converted into steady motion.

x having been determined in terms of ϕ, $x+\phi$ is to be deemed a known function of ϕ, $f(\phi)$ suppose, which will have 2π for its period. The coefficients A_n may then be deemed known, and on substituting in (2) we shall have y expressed in terms of $f(\phi)$. Denoting this expression for y by $F(\phi)$ we shall have

$$F(\phi)=\frac{1}{\pi}\Sigma\int_0^{2\pi}\frac{e^{nk}-e^{-nk}}{e^{nk}+e^{-nk}}\cos n\phi\sin n\phi' f(\phi')\,d\phi', \ldots(3)$$

where of course the integration with respect to ϕ' may stop at π if we double the coefficient, since $f(2\pi-\phi)=-f(\phi)$.

If the trial curve had been the true outline, the curve of which the ordinate is determined by (3) would have come out identical with the original, which would have proved the original to have been correct: otherwise the new curve will be a much closer approximation to the true form than the trial curve, and may be used as a fresh trial curve, and so on.

In (3) the integration is supposed to be performed first and then the summation. If we attempted to perform the summation first, we should encounter a series which is neither convergent nor divergent, but fluctuating. Such a series may however be summed by regarding it as the limit of the convergent series formed from it by multiplying the nth term by the nth power of a quantity less than 1, and which is supposed to become equal to 1 in the limit. The summation cannot however, so far as I know, be actually effected except in two cases.

The first case is that of a fluid of infinite depth, for which the fraction involving the exponentials becomes equal to 1, and the series divides into a pair of series of sines of arcs in arithmetical progression, which may be summed by regarding it as the limit of another series; a view to which we are naturally conducted by regarding the stream line of the surface as the limit of a stream line taken first a little below the surface. The other case is that in which the wave length is regarded as infinitely great instead of infinitely small compared with the depth of the fluid. In this case we first take a crest for the origin of x and then make λ infinite, when the sum takes the form of a definite integral which may be evaluated according to the known formula

$$\int_0^{\infty}\frac{e^{ax}-e^{-ax}}{e^{ax}+e^{-ax}}\sin bx\,dx=\frac{\pi/a}{e^{\pi b/2a}-e^{-\pi b/2a}}.$$

As the other crests have moved off to infinity, we are in this case left with a solitary wave.

The results in the two cases are as follows:—

1. Case of oscillatory waves in a fluid of infinite depth

$$F(\phi)=\frac{1}{2\pi}\int_0^\pi\left\{f(\phi+\chi)-f(\phi-\chi)\right\}\cot\tfrac{1}{2}\chi d\chi.$$

2. Case of a solitary wave

$$F(\phi)=\frac{1}{k}\int_0^\infty\{f(\phi+\omega)-f(\phi-\omega)\}\,\frac{d\omega}{e^{\pi\omega/2k}-e^{-\pi\omega/2k}}.$$

In the former case, partly as a severe test of the method, and partly for other reasons, I took as the trial outline a serrated line composed of straight lines inclined at angles[1] of $\pm 30^\circ$ to the horizon, giving ridges formed of wedges of 120°, and troughs formed of similar wedges inverted. Even in this case, where the assumed form is so egregiously wrong as regards the troughs, the first approximation led to a form presenting ridges of 120°, which for a considerable way down hardly differed from the original, while in lieu of the angular troughs I got a curved outline, with a depth from crest to trough of about 0·22 of the wave length, instead of $1/2\sqrt{3}$ or 0·309 as in the serrated outline assumed for trial. The results of some other trials are encouraging as to the success of the method, but as I mentioned already the numerical calculations are not finished.

(2) *On the Crystallography of Miargyrite.* By Professor W. J. Lewis. With Plate X.

[*Revised and enlarged.* *July* 19, 1883.]

In the description of this mineral given by Naumann, in *Pogg. Ann.* XVII., 1826, the planes fall into three prominent zones, those denoted by him [*abo*] (the zone of symmetry), [*bfidst*] = [100, 111], and [*opg*] = [10$\bar{1}$, 31$\bar{3}$]; and the cleavages are stated to be parallel to *b* and *m*. In Miller's *Mineralogy* (1852) two new zones [*ζhr*M*xy*], [*vzkty,g*] are added to those already given by Naumann. Owing to the relation between the angular elements, the angles in the zone [*ζhr*M*xy*] approximate closely to those in the zone [*bfidst*]. Miller retains the orientation and axial system adopted by Naunann, but he has misplaced the angles *ao* and *bo*, possibly owing to a confusion in changing the letters used to denote the faces. For Miller always uses *a*, *b* and *c* in the oblique system to denote the poles (100), (010), (001). Hence Naumann's *b* becomes

[1] In the case of a simple serrated outline, $f(\phi)$, and therefore $F(\phi)$, is independent of the assumed inclination.

Miller's $A_{,}$, and his a becomes Miller's C.[1] But whilst Naumann gives $ao = 48^\circ\ 14'$, ob (cleav.) $= 50^\circ\ 10'$; Miller has $oA_{,}$ (cleav.) $= 48^\circ\ 14'$, $OC = 50^\circ\ 10'$. Now as Miller clearly follows Naumann's orientation he has fallen into error in thus transposing these angles. Attention was called to this transposition by Prof. Weisbach in a paper on Miargyrite published in *Pogg. Ann.* cxxv., 1865, in which he gives Miller's new planes without any criticism which would connect them with this transposition. Now, if Miller, as seemed probable, had obtained his planes by measurement, it follows that his zone [CζhrMx] will be incorrectly or correctly placed according as C is or is not the cleavage plane. It will be coincident with Naumann's zone [*bfdst*] if the plane C be that which Miller found to be the cleavage plane. In this case his statement that A is the cleavage plane would be simply due to a misapprehension that this plane was still Naumann's *b*. Starting with Miller's data, the determination of the crystals presents considerable difficulty, for the angles in the common prominent zone [*bfdst*] are liable to such great variations as to render the discrimination of this zone from Miller's [ζhrMxy] often very uncertain. By, however, attending to the physical character of the faces and the traces of cleavage, when they could be perceived, I found that all the crystals measured by me (about twenty in number) led to the result, that the zone [*bfdst*] was the one always present, and that in no case was [ζhrMxy] prominent. It seemed certain, moreover, that Weisbach's suggestion, that a transposition of the angles *ao* and *bo* had been made by Miller, was correct. In this case then the zone [CζhrMxy] given by Miller is simply the common zone [*bfdst*], which has acquired the position given it by the transposition of Naumann's planes *a* and *b*. Moreover the zone [*vzkty*] will also change its position with the same transposition, and these planes will have for indices v (013) = Weisbach's β, z (137), k (124), y $(21\bar{1})$ instead of those given by Miller. A further consequence of this error is that all the angles not in the zone of symmetry, calculated by Miller, will be incorrect, and this might serve to conceal the divergence in some of the observed angles from their values as given by calculation. A search through Prof. Miller's manuscript books was rewarded with the discovery of the actual measurements of a crystal which justify the conclusion arrived at. He finds $50^\circ\ 20'$ to be the angle between *o* and the cleavage plane, to which he has attached the letter *c*. He also gives a rough drawing of the crystal projected on the plane of symmetry, which is sufficiently characteristic to render its identification easy, but the

[1] I employ capital letters in the early part of this paper to distinguish Miller's letters, whenever they are liable to confusion with those of others. In the actual discussion of my own observations, capitals, when used, represent planes for which no other suitable lettering exists.

most careful search for it has been so far fruitless. Its habit is like that of the crystal (No. 7) measured by me, but it had certain planes not conspicuous on the latter crystal. I have reproduced his drawing (Fig. 14) as nearly as possible. In Fig. 15 I have repeated this drawing as it is seen through the paper with the plane (100) placed in the vertical. The crystal was probably a fragment and only showed the faces given, which correspond with the faces *below* the paper in the stereographic projection, as is also the case with crystal 7. Miller partially measured a second crystal belonging to Brooke which I have denoted by D in the tables. The solution seems not to have been completed; and the diagram accompanying the observations is too rough to render the identification of the crystal easy, but it seems to be similar to crystal 3. As far as shown it was a combination of $\eta dst\omega x\beta zk$. The measurements of $t\omega = 16^\circ\ 14'$ and $\omega x = 9^\circ\ 4'$ are underlined as if they were from good observations. The zone $[\beta x t_,]$ is also measured.

Whilst my manuscript was in the press I obtained from Mrs Miller a number of loose papers which contain additional measurements by Prof. Miller. These papers contain the complete solutions of two crystals, *A* and *B*. *A* is a crystal with well-developed *x* and *g* planes. It does not however correspond with any of the crystals named by me, though its habit is much that of No. 14. The crystal *B* is clearly identical with my No. 7, and Miller's measurements agree closely with mine. Another loose sheet contains a diagram, labelled *C*, identical with that in the note-book. The crystal-faces were denoted by Greek letters during measurement, and these letters were afterwards replaced in ink by the letters employed to denote the faces in Miller's *Mineralogy*. The crystals are all similarly placed in the diagrams, so that, as already stated, the faces shown are those *below* the paper in the stereographic projection. The zone of symmetry was measured on *A*, but the angles seem to be very unreliable, as the readings are in several cases enclosed between four or five brackets, and I have come to the conclusion that Prof. Miller was in the habit of denoting unreliable readings by enclosing them in brackets, and that the number of brackets denoted the degree of unreliability. This zone was not measured on *B*; the readings for *s*, *t*, *ω*, *x* are underlined, and the measurements are identical with those made by me. These measurements have been omitted in my tables; but all the other important angles given by Miller are appended under the headings *A*, *B*, *C*, which refer to the three crystals so labelled. Miller also measured two or three other crystals, but the solution is in no case completed. Amongst these No. 9 was recognised both by the diagram and by the identity of the angles with those obtained by me. Miller seems, however, to have been somewhat puzzled by the plane *i* on this crystal, and not to have measured

the angles in the zone of symmetry. There are also one or two stereographic projections which show that Miller had at one time contemplated a change in the parametral plane. In one case d is tried as (111), and in another β (Miller's v) as (110). Miller seems to have made the observations given in the note-book in the summer of 1848, and as the crystal there described is labelled C I should expect that the loose papers bear the same date. In this case Miller must have felt too little confidence in the results of his own observations to justify an alteration of Naumann's elements, and has contented himself with giving the planes which he believed to be new. The plane e (212) does not seem to have been observed by him. A plane given by Miller on crystal A in zone [dst], and making an angle of 16° 5′ with a (100) falls between (11 11) and (10 11). The former is inclined to a at an angle of 15° 39·25′, the latter at one of 17° 6′·5.

In Weisbach's paper in 1865 the axial system previously adopted is exchanged for one in which o and c become the orthopinakoid and base. The object aimed at in this change is to be able to represent the form g ($\bar{3}13$) as a prism. It has, however, serious disadvantages which more than compensate any advantage so obtained, and does not seem justified by the importance of the form ($\bar{3}13$). The zone [og] is generally a much striated and curved zone, the angles in which are rarely capable of even approximate determination. The planes x are those which are generally most largely developed, and they were very preponderant in the crystals measured by Miller. The planes β (013) are frequently very largely developed, as are also the planes ξ ($\bar{2}13$). Naumann's orientation places the well-marked plane A and the zones [Adst] in prominent positions which show the development well; and both the development of the more complicated crystals and the simplicity of the indices is decidedly in favour of retaining the axial system first selected. The letters employed to designate the faces have been much confused, and it is not possible to find a lettering free from objection. I have tried to retain as far as possible the lettering used by the various writers. The letters z, k are used to denote the planes (137), (124) in their correct positions, and several of the letters applied by Miller in the zone [ct] have been transferred to new planes as they no longer refer to any known planes.

Whilst completing the calculations involved in the determination of the elements I received through the kindness of Professor Groth a copy of the proof-sheets of a paper on the mineral by Prof. vom Rath.

Prof. vom Rath has in this paper deduced elements from the spherical triangle $ss_{,}g$; and has given no weight to any other

of his measurements. He seems not to have measured many crystals, and, however definite the images he may have obtained from the faces s, $s_{,}$, g, one feels somewhat doubtful as to the reliability of elements so obtained, when the other measurements on even the same crystal diverge largely from their calculated values. Moreover the angles he found differ much from those found on any of the crystals examined by me. Prof. vom Rath gives $ss_{,} = 77° \, 4'$; $gs = 69° \, 54'$; and $gs_{,} = 53° \, 33'$. From this triangle it is possible to obtain the angles between three theoretical planes in the zone of symmetry, and hence the elements of the crystal. The elements are therefore obtained in a very indirect way, and any errors in the angles will run great risk of being multiplied in the course of the calculations. I have seldom found the planes s give definite images, and the planes g rarely gave images sufficiently definite to be observed with a telescope.

The crystallography of the mineral is now seen to be much simpler than that given by Miller. I find the principal zones to be [A*o*C*m*], [A*fdst*], [*opg*], [A*h*ξ*g*], and [*βzktσg*]. The crystals, however, vary much in appearance according as some planes preponderate over others or not, and the actual planes which so preponderate. As far as they can be placed under types I should be inclined to class them as follows:

(1) A type common at Braünsdorff in which the planes *a*, *o*, *c* are large, and *d*, *s*, *t* appear in diminishing magnitude giving the crystals at the edges in which the two adjacent zones [*dst*] meet a sharp wedge-shaped appearance. This type may be subdivided into one in which the planes *d*, *s*, *t*, &c. are well developed, and a second subdivision in which the striations are so prominent that the successive planes seem to flow into one another, and we have a couple of apparently curved wedges terminating the crystals on opposite sides of the zone of symmetry. Figs. 2 and 3 represent crystals of these forms. (2) Crystals resembling the figure given in Naumann's *Mineralogy* in which the planes in the zone of symmetry and in [*dst*] are all well developed; and the plane ξ ($\bar{2}13$) has been found by me to be prominent (see Fig. 4). (3) A type in which with the planes *a*, *o*, *c* large, the planes *d* and *g* are about equally developed, giving the crystals a somewhat rhombic appearance as is shown in Fig. 5. (4) Crystals with numerous planes in [*dst*] and in [*βzkt*], all about equally developed. The crystals of this type are common and somewhat difficult to decipher without measurement. They differ moreover considerably the one from the other, inasmuch as in some crystals the planes *x* will be largely developed, in others the planes *β*, and again in others one large *x* plane will be associated with a large *β* plane lying on opposite sides of the [*dst*] zone not containing the large *x* plane. Figs. 6, 7 and 8 are representations of crystals of this kind. (5) Crystals in which *c* is

very large, and we obtain somewhat flat crystals in which planes g &c. in the zone [opg] are fairly conspicuous. The zones [dst] in these crystals have less prominence than in other types (Figs. 9 and 10). (6) Crystals of the habit of Kenngottite in which a large c plane is associated with minor planes which lie either in the zones [dst] or in [opg]. Fig. 11 is a representation of a crystal of the latter kind which seems to be rare. (7) Small crystals with o, b well developed, and planes g forming a prism. The plane a in the best instance I have observed (given in Fig. 12) becomes a triangle bounded by the zones [dst], which are inconspicuous, and c was absent.

As is well known the faces in the zones [aoc] and [dst] are all much striated, the latter in the direction parallel to their common intersections. The plane a has two sets of striations parallel to the intersections with the adjacent d planes and sometimes striations parallel to [ao]; and it in consequence always gives an image from which radiate the branches of a four or six-rayed cross. The plane o gave very good images in a few cases, but the plane c generally gave two images. The plane ξ is one common to several of the crystals examined by me, and is a well developed face: it gives reliable though not perfect images. Of Miller's zone [ζhrt] only the plane h has been found associated with those common to [aoc] and [dst].

I have retained the axial system adopted by Naumann and Miller. The axial elements as obtained by direct observation are too little reliable to be of much use, and the best angles are such as are ill adapted for obtaining them by calculation. I have brought into the determination of these elements all the angles which seemed fairly reliable, where the process of calculation did not seem to reduce the value to that of a poor observation. The angles so employed amounted to thirty-six. It is naturally extremely difficult to attach weights to the observations in a satisfactory manner, but an attempt has been made to do this in the case of the best angles. Owing to the peculiar development of Miargyrite crystals, the spherical triangles one has to use are of the worst description for purposes of calculation, and great care has been necessary to prevent multiplying, to a serious extent, the errors which necessarily must exist in the angles used. Whilst examining these errors and variations, I noticed some peculiar discrepancies which seem not uncommon. On crystal (No. 7), which seems well developed and which gives good reflexions, the angles st, $t\omega$, ωx, on measurement were found to be $13^\circ 51' \cdot 5$, $17^\circ 24'$, $9^\circ 10' \cdot 5$. The two latter agree well with those calculated from the elements, but the first ought to be $14^\circ 16' \cdot 33$. Now criticism of the possibility of these four planes including these angles between them and having a constant anharmonic ratio leads to the

following equation between the errors in the three observations. Let the errors in xt, ωs and ωx be $d\phi$, $d\psi$ and $d\alpha$ respectively;

Then $$1{\cdot}51\, d\phi = 2{\cdot}56\, d\alpha + {\cdot}6 d\psi,$$
whence it is obvious that the best method of using the angles is to calculate ωx from xt and ωs; and that the most disadvantageous combination is to calculate ωs from the other two.

Again in crystal 3 a well developed plane (δ) which gives a good reflexion is present in one of the zones [*dst*], and which is inclined to *d* at an angle of $1^\circ 24'{\cdot}5$. The true plane *d* is itself inclined to *s* at an angle considerably less than that usually found. For the adjacent plane δ no indices lower than (17 6 6) can be found which at all fit with the results of observation. The angle (*d*, 17 6 6) given by calculation is $1^\circ 37'{\cdot}5$. The angles in the other zone [*dst*] are very near the normal angles obtained from the elements. The angle $dd_{,}$ is, however, much below that common on other crystals, and this will clearly be connected with the dislocation in the zone [$d\delta st$]. The presence of this plane δ in a well defined zone shows how difficult it will be to obtain reliable measurements where such variations are rendered less distinct by stronger striation.

The measurement of all the crystals was first made with a small goniometer made by Cary and supplied with a mirror for giving a faint-signal line. When the images were bright and definite enough, the image from the face was brought accurately on the faint signal when both were viewed through a small Galilean telescope held in the hand. Good observations so obtained would be underlined once, twice or thrice, according to the goodness of the readings. The better angles were then in most cases measured with a large horizontal goniometer (200 m.m. in diameter), to which a telescope (magnifying from 10—12 times) and collimator had been fitted. It was found that the readings obtained from this larger goniometer, which reads to 20″, differed from those given by the smaller instrument by only a fraction of a minute, whenever the angle measured by this latter had been twice or thrice underlined. Three or four of the angles on the best crystal (that represented in Fig. 2) were also measured with a Fuess' goniometer in the British Museum, when the extreme variation from those previously obtained by me was $1'{\cdot}5$. This difference probably arose from the difference in the images as magnified by the telescopes. The angles so compared were:

	Miller's Goniometer.	Cary's Goniometer.	Fuess' Goniometer.
$dd_{,}$	$96^\circ\ 26'\frac{1}{4}$	$96^\circ\ 26'\frac{1}{3}$	$96^\circ\ 24'\frac{3}{4}$
dk	35 9	35 4	35 $3\frac{3}{4}$
$k\xi$	34 $20\frac{1}{2}$	34 $24\frac{1}{3}$	34 $24\frac{1}{4}$
$d\xi$	69 $29\frac{1}{2}$	69 $28\frac{1}{6}$	69 28
ds	10 $31\frac{2}{3}$	10 $31\frac{1}{2}$	10 $31\frac{5}{8}$
st	14 31	14 27	14 $26\frac{3}{4}$

The images from k and t are multiple, and the faces are also less bright than the others, so that the angles involving these faces would be little suited for testing the accuracy of the several instruments. The comparison of these angles, and of a large number of angles, measured with both the large horizontal goniometer and the Cary's goniometer, has given me confidence in employing several of the angles obtained with the latter instrument only, when the images were definite enough to give good observations with the small Galilean telescope, but were too faint to allow the higher magnifying telescope to be used. In the accompanying tables of angles those measured by the horizontal goniometer, with telescope and collimator, are distinguished from the others by being printed in thicker and larger type. Two angles given together are those obtained from the corresponding pairs of faces; but if the second angle is enclosed in brackets one of the faces at least gave two images and the angles are alternative, the enclosed one being the least trustworthy. The planes observed on the various crystals and the zones connecting them are shown in Fig. 1. A few of the letters and of the corresponding zone-circles have been omitted to diminish the complication of the diagram.

The planes observed on the crystals examined by me are a 100, m 101, L 703, λ (102) ? (105)? (104)? c 001, M $\bar{1}03$, o $\bar{1}01$, R $\bar{2}01$, N $\bar{3}01$, (711), η 611, f* 922, ϕ 411, d 311, ϵ 522, s 211, t 111, X 122, ω 011, x $\bar{1}22$, σ $\bar{2}11$, i $\bar{3}11$, b 010, r 121, h 113, β 013, ξ $\bar{2}13$, g $\bar{3}13$, ψ $\bar{4}13$, q $\overline{12}\,1\,3$, z 137, k 124, χ $\bar{2}12$, γ $\bar{4}14$, π $\bar{5}15$, p $\bar{6}16$, w $\overline{12}\,1\,15$, ζ $\bar{2}15$, Δ 210, (1 6 16) ? ($\bar{1}19$)? ($\bar{1}\,2\,10$) ? (139) ?

Besides these n 301, μ $70\bar{2}$, u $\bar{2}03$, χ 15 1 1, ω 811, F 511, δ 13 4 4, A $\bar{1}11$, ς 181, E 212 (?), I $\bar{6}76$, e $\overline{12}\,5\,20$, γ $\overline{36}\,13\,39$(?) α† $\bar{2}33$ (?), have been recorded by other observers.

The angles I have adopted for the purposes of calculation are co $48^{\circ}\,21'\cdot17$, $a_{,}o$ $50^{\circ}\,16'\cdot25$ and $dd_{,}$ $96^{\circ}\,27'\cdot2$. From these the

* This plane f (922) was observed and so lettered by Naumann.

† α is in all probability identical with x. It is largely developed in Weisbach's Fig. 9. His measurements are only approximate and do not accord well either with α or x. He gives

	W. calcd.	calcd. for x.
$\alpha\alpha_{,}=40^{\circ}\frac{3}{4}$	$42^{\circ}\,8'$	$89^{\circ}\,48'67$,
$\alpha\alpha=98^{\circ}\frac{1}{4}$	$99^{\circ}\,20'$	$96^{\circ}\,18'$.
$s\alpha$ $42^{\circ}\,47'$		

It has been also observed by vom Rath, who gives no measurements but calls attention to the remarkable divergence he finds in the measurements of it, and also to its great development on several crystals. The plane x was well developed on nine of my crystals. It lies in the zones [mk], [$t_{,}v$], [$ck_{,}$], [$hs_{,}$], [$g\sigma_{,}$] and [R$t_{,}$]. These zones give a ready means of testing the true indices to be assigned to the plane, when the angles in the zone [dtx] are too little reliable to distinguish between the two sets of indices.

Millerian elements—*am* $41^\circ\ 23'\cdot79$, *bt* $27^\circ\ 24'\cdot4$, *cm* $39^\circ\ 58'\cdot79$;—have been calculated. These are equivalent to

$$a : b : c = 3{\cdot}0017 : 1 : 2{\cdot}9166;\ \eta = 81^\circ\ 22'\cdot58.$$

The forms observed on the several crystals were the following:

Crystal 1. Fig. 2.

a, η, d, s, t, b, m, (105), *c,* M, *o,* R, *ψ, ξ, h, k.*

This is a remarkably good crystal; *ξ* and *k* are fairly well developed, the former gives good images, the latter poor ones: *ψ* and *h* are very minute, and reflexions are obtained in the zone [*aξ*] between *ξ* and *ψ*, giving angles $\xi\,\Theta$ $3^\circ\ 22'$ or $4^\circ\ 15'$ or $4^\circ\ 56'$.

Crystal 2. Fig. 4.

a, η, f, φ, d, s, t, ω, c, o, N, q, ψ, g, ξ, h, w $(12\ 1\ \overline{15})$.

A good crystal on which *ξ* is the most prominent plane on one side. A plane $(12\ 1\ \overline{15})$ is well developed on this crystal. The plane *g* is badly developed, and gives several images, so that *ag* may be $59^\circ\ 50'$, $60^\circ\ 8'$, $60^\circ\ 18'$, $61^\circ\ 15'$. The planes *q* on opposite sides of N are not in a zone with N as was determined by measuring $qq_{,} = 31^\circ\ 51'\tfrac{1}{4}$ (poor). There is also a badly developed plane giving an indefinite reflexion lying between *q* and *ψ*, so that $a\Lambda = 40^\circ\ 53'$ nearly. This plane may be therefore be $(\overline{11}\ 26)$. For $(\overline{11}\ 26)$ we have $(a, \overline{11}\ 26) = 40^\circ\ 51'\ 33$.

Crystal 3. *a, d, s, t, ω,* (105), *c, o, g, p,* or $(\bar{7}17)$.

There is a rough pitted plane in the zone [*adst*] on this crystal which may also possibly be in a zone [O*g*]. In this case it would be $(\bar{1}11)$.

In the zone [$ad_{,}s_{,}$] is the plane *δ* (17 66) already mentioned.

Crystal 4. Fig. 8.

a, f, d, s, t, x, σ, i, L, *m, λ, c,* M, *o, p, γ, π, g, h, β, z,* (16 16) ?

The planes are all small, and the crystal top has a rounded aspect from the number of small faces. The planes in the zone [*og*] are all much rounded and hardly allow of exact determination. On one side images extend in an almost continuous band through an interval of 17° with brighter portions at intervals of about one or two degrees.

Another crystal of much the same development was considerably distorted, and gave very poor measurements. It showed the planes R, *k*, *ω* in addition to those on the former. The zone [R*tx*$_{,}$] was measured.

$$\left[\begin{array}{ll} x_{,}t = & 39^\circ\ 36' \\ t\mathrm{R} & 63^\circ\ 46' \\ & (64^\circ\ 25') \end{array}\right.$$

Crystal 5. Fig. 5.

a, d, s, ω, b, c, o, p, g.

Also rough pitted planes in zone [*ds*] adjoining *b*.

The planes are all largely developed except *p*, *s* and *ω*, but they all give very poor reflexions. The corresponding planes *o*, *g* do not lie accurately in a zone.

Crystal 6. Fig. 10.

a, η, φ, d, t, ω, x, σ, i, o, c, β, z, k, g, $(\overline{12}\,1\,12)$, $(\overline{21}\,4\,21)$ or $(\bar{5}15)$, *h.*

There is a doubtful plane *ρ* $(1\bar{3}9)$ on this crystal lying between *c* and *z* and in the zone [*ahgβ*].

Crystal 7. Fig. 7.

s, t, ω, x, σ, b, c, m, o, β, z, k, h.

There is a rough drawing of this crystal by Miller in a note-book belonging to Brooke, which is a copy of that on the loose papers already described; and there is the following note by Miller. "It is not worth while to attempt to give angles till all the crystals are measured; v, u, t, x, y and z are new." The planes extant are those below the paper in the stereographic projection, and to show these above by turning the crystal faces towards the observer naturally places them in such a position as to encourage Miller in his mistake. The cleavages *a*, *m* are fairly distinct.

Crystal 8.

a, c, M, *o, β, k, s, t, x, σ, i, g, p.*

The planes on this crystal are all much striated, and the measurements obtained were poor. They, however, establish clearly the tautozonal relations of the plane *x*.

Crystal 9. Fig. 16.

a, o, c, η, φ, d, s, ω, x, σ, i, β, z, k, p, g.

A small fragment with *z* and *k* comparatively large. It was measured by Miller, but the solution was not completed. The measurements agree closely with mine.

Crystal 10. Fig. 12.

a, o, b, g, d, s, t.

A very small crystal with distinct prismatic development, and large *o* and *b* planes.

Crystal 11. Fig. 6.

d, s, t, ω, x, σ, i, g, k.

A fragment imbedded in massive miargyrite. The planes are all much striated.

Crystal 12. Fig. 11.

c, o, a, m, π, g.

A small crystal, with *c* large; and zone [*o*, π, *g*] striated, and the planes *o*, π alternating a good deal.

Crystal 13. Fig. 3.

a, c, M, *o, d, s, t.*

This crystal and several like it measured by me are somewhat large crystals with large *c* planes, and much striated rounded planes *d, s, t,* terminating the crystals at both ends in sharp trigonal wedges. The measurements obtained on these crystals (one of which showed the plane *k*) were only of value in determining the forms actually present.

Crystal 14.

a, d, ε, s, t, X, ω, x, m, (205) ?, (104), *c, o, p, g.*

A good crystal still attached, with others, to a matrix of quartz, closely resembling specimens from Braünsdorff. It is however accompanied by a label which gives the locality Wolfsberg. Owing to its attachment to the matrix, but few zones could be measured, and these only approximately. From the habit of the crystals on the specimen it seems probable that the crystal *A* measured by Miller was removed from this specimen. The zone [*opg*] is largely but irregularly developed, whilst the zone [*dεst*] seems almost curved from the number of planes. Amongst these ε and X are largely developed. In the zone [*ca*] the plane *o* is alone large, *a* and *m* form simply a notched edge bounding the plane *d*. The following are the principal angles obtained.

	calculated.	found.		calculated.	found.
dε	4° 56′	4° 39′	*oc*	48° 21′·17	48° 20′
		(9 15)	*c* (104)	13 2·6	13 46
ds	10 45·5	(11 20)	*c* (205) ?	19 57·3	20 29
		11 32			(21 35)
st	14 16·33	14 18	*cm*	39 58·8	40 20
*t*X	8 21·33	8 10	*ma*	41 23·8	41 9
X*ω*	9 2	9 11			
ωx	9 6	9 38			

On a loose crystal of similar habit the following forms were observed:

a, d, s, t, ω, x, m, o, p, g, r 121, 12 7 $\bar{1}$?, 836 ?

These three latter planes are fairly well developed. The angles obtained were however not good, and I feel no great confidence in the indices I have assigned to the two latter planes, as they had not the certainty obtainable when they are deduced from the

intersections of known zones. The same reason has led me to doubt the indices assigned to most of the planes to which a note of interrogation is attached. The indices were in these cases generally deduced from measurements of the angles made with prominent planes in the zone of symmetry.

Crystal 15. Fig. 13.

A twin-crystal of habit and forms given under crystal 13. The one portion is drawn with the axial system used in Figs. 4 and 12. The portion, whose faces are denoted by the Greek letters equivalent to the italics on the first, is obtained by rotating the first axial system about the normal to ξ ($\bar{2}13$). The crystal is imbedded in the midst of others, and only so much as is shown in the figure can be seen. Measurement was difficult owing to the want of lustre and the depth of striation on the planes *a*, *d*, *s*, and the unevenness of *c* and γ. There was a smaller twin of the same kind on the specimen.

Such approximate measurements as were obtained are compared in the following table with those calculated on the supposition that the individuals have ξ ($\bar{2}13$) for twin- and composition-face.

	Calculated
$c\gamma$ 76° 23′ fairly good (76° 56′)	76° 2′
$a\alpha$ 38° 45′	39° 54′
$c\alpha$ 55° 51′	55° 51′
$ca = 81°$ 18′ near	
$\gamma\alpha = 81°$ 30′ „	
ct 70° 38′ „	
[ad 44° near $= \alpha\delta$	
dt 26° – 27° near $= \delta\tau$	
$d\delta$ 40° – 42° very doubtful	40° 58′
$t\tau$ $84\frac{1}{4}°$ – $86\frac{3}{8}°$ „	83° 56′

Two small crystals, supposed to come from South America, were recently measured by my friend Mr H. A. Miers of the British Museum. He has kindly communicated to me the results he obtained. The forms observed were *acogdstωσikξ*. The crystals were much striated and distorted, *k* being the best face, and the faces which give clear definite images were *cotωσk*.

Since finishing my calculations I have to thank Professor Groth for sending me the specimen in the Strassburg collection, measured by Mr Friedländer, and of which a figure is given in his catalogue. It consists of two very fine crystals attached together. Besides the forms quoted by Mr Friedländer the forms β (013) and ζ ($\bar{2}15$) are well developed on the crystal. Traces of *b*, η, *x*, and *k* were also

perceived, and I believe that there are three other planes $(\bar{1}\,2\,10)$, $(1\,1\,9)$ and one in the zone $[c\beta]$ not already given, to be found on it. In Mr Friedländer's drawing the crystal is represented as hemimorphously developed, inasmuch as γ is preponderant on one side of the pole of symmetry, whilst g is so on the other. I do not think this quite the case, and I was most struck by the very large development of one of the planes χ on the smaller crystal of the group. Mr Friedländer calculated his elements from angles which involve c, the reflexions from which I do not think very trustworthy as the planes are deeply striated. He also gives the angle $a_{,}o = 49^\circ\ 45'$, a value which on careful examination I am inclined to doubt. I obtained an angle nearly the same as this, but the face a, which gives this value, is intersected by a crack, and one portion has been dislocated from its true position. This dislocated portion is that which gives the brightest image and the value $49^\circ\ 45'$; but that it is not the true face is shown by its being very far from parallel to the opposite a, and also by the image being outside the zones which contain a. The image from the undisturbed portion is less distinct, but it gives $a_{,}o = 50^\circ\ 4'\cdot5$, $\underline{50^\circ\ 3'}$, whilst the angle between the parallel faces was found to be $50^\circ\ 11'\cdot5$ or $\underline{50^\circ\ 10'}$. He also gives $ai = 47^\circ\ 39'$. The angle to the dislocated portion of a was found by me to be $47^\circ\ 33'$; that to the true a $47^\circ\ 40'$. Mr H. A. Miers has confirmed my results with the Fuess' goniometer and observed a new plane Δ (210) lying between s and σ. He found ao $55^\circ\ 5'\cdot5$, $(50^\circ\ 8')$, $50^\circ\ 9'$; $\chi\chi_{,}$ $83^\circ\ 29'\frac{2}{3}$; $\chi\zeta$ $27^\circ\ 22'\cdot5$. This crystal is a combination of

$a, \eta, d, s, t, \omega, x, \sigma, i, o, \gamma, g, \chi, b, \beta, \zeta, k, \Delta\ (210), (\bar{1}\,1\,9)?\ (\bar{1}\,2\,10)?$

The measurements obtained are given in a column headed "Strassburg."

In the accompanying tables I have given the principal angles which I have calculated and observed. The first column contains the calculated values, the other columns give the angles observed on the crystals denoted by the number at the head of the column. Those printed in dark thick type were observed with telescope and collimator, the others with the small vertical goniometer. Where several observations are given on one crystal, they are from corresponding faces, unless they are enclosed in brackets, when they are alternative and due to more than one image on one or both faces.

In Figs. 2, 3, 5, 8, the plane of symmetry is nearly, but not quite, coincident with the plane of the paper. Figs. 6, 9, 10, 14, 15, 16, A and $A_{,}$ are orthogonal projections on the plane of symmetry. Figs. 14 and A are moderately close copies of Miller's diagrams of crystals C and A. Figs. 15 and $A_{,}$ are repetitions of these diagrams

as seen through the paper and turned round so as to bring them into the normal position. Fig. 7 is an inverted copy of the planes on my crystal 7 and Miller's *B*. Miller's diagram is very like this, excepting that none of the planes in the zone of symmetry are shown. The axial system used is the same as in Fig. 2. In Figs. 4, 11 and 12 the plane of symmetry is vertical and directed back and fore. Fig. 13 represents the twin-crystal, and has been already described.

	Calculated.	1	2	4	11	Strassburg.
	° ′	° ′	° ′	° ′	° ′	° ′
cζ	36 20·75	......	......	......	......	36 21·5 36 26·5 (36 36)
cξ	51 58·5	51 56	51 45·6			
cχ	63 45	......	......	......	......	63 43·16 63 46 (63 56)
ξσ	26 28					π − 116 15·25
χσ	14 41·5	......	......	......	14 59·5	14 39·4
σs,	31 59·9	......	......	31 40 31 43·5	31 48·67	31 55
s,Δ	15 38	......	......	......	......	15 28·5 (15 7)
ξs,	58 27·9	58 32·33	58 33·16			
s,c,	69 33·6	69 27	69 29·75	......	......	69 34
				On 6.		
aq,	23 43·5	......	23 57			
aΛ	40 51	......	40 53 (poor)			
aψ,	50 16	50 16	50 3			
ag,	59 12·5	......	60 8 (60 19)	58 41·5		
aξ,	70 8·6	......	(61 13) 69 55			
a,ξ,	109 51·4	109 46	110 2 (109 52)			
g,β,	37 1	......	......	37 2·5		
ξ,β,	26 4·9					
β,h,	12 47	......	......	12 46		
ξ,h,	38 51·9	38 49	38 50			
a,h,	70 59·5	70 57				
h, ($\bar{1}3\bar{9}$)		......	......	8 4		
			On 7.		Miller. C	
ch	44 1	......	......	43 57		
ht	25 19·6	......	25 20·33	25 20		
ct	69 20·6	69 17·5	......	69 16·16	69 37	

	Calculated.	1	2	3	4	5	6	7	12	Strassburg.	Miller's measurements. A	Miller's measurements. C
	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′
an	17 50·07	......	......	......	......	......		......	......	......		20 0 = *a, cl.*
aL	22 37	......	......	......	22 4							
Lm	18 46·8	......	......	......	19 7 (19 43)							
am	41 23·8	41 26·5	41 22·25	41 17·5	41 12·5	......		......	41 5	41 18·5 41 25	41 28	38 5 = *a, cl.*
a(105)	71 47·25	70 29 (72 41) (73 31)	......	71 38 (72 1)								
λc	24 7·16	......	......	......	24 21 to 27 25 (26 51 best)							
mc	39 58·8	39 46	39 56·75 (39 58·33)	......	40 29	......		40 1	39 53	39 58	40 54	
ac	81 22·58	81 21 81 15	81 19 (81 21)	81 20	......	80 27	......	......	80 58	81 23 81 29 81 38·33		81 28
ca,	98 37·42	98 43·5 98 42	......	......	98 12·5	......	......	......	......	......		99 32
cM	18 36·1	18 27	......	......	18 12·5	......	18 35					
Mo	29 45	29 54	29 42·67	......	29 44	......	29 45					
*co**	48 21·17	48 20 (48 21) 48 3	......	......	47 56	48 55 48 41	47 40	48 26	48 21	48 17·33 48 17·16 48 22	49 45 (*poor*)	
oN	30 35·65	30 54	30 35	......								
Na,	19 40·6	19 40 19 18	19 44·5	......								
Ra,	28 52	28 56	......	......								
*oa,**	50 16·25	50 22 50 41	50 19·33	50 9·5	50 17 50 7·5 (50 22·5)	50 43	......	49 16·5 (cleavage, poor)	50 14	50 3 50 10 50 19		50 20 = *o clea.*
om,	91 40	......	......	......	......	......	......	91 47·5 (cleavage)				

	Calculated.	1	2	3	4	6	7	8	9	10	11	Strassburg.	Miller's measurements. A	C
	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′
a (711)	23·45	......	......	......	......	23 31·5								
$a\eta$	26 56	26 52	......	......	......	27 51	......	......	27 25	......		......	26 27	......
ηd	17 50·23	......	......	......	......	17 44·5	......	......	18 7	......		......	17 20	......
$a\phi$	36 58·67	......	36 45·83 (36 53·33)	......	......	38 5·5	......	......	......	......		......	36 51	......
ϕd	7 48·56	......	7 47·67	......	......	7 30	......	......	7 50				6 56	
ad	44 46·23	44 48·5	44 33·5 (44 41)	44 56·5 44 47	......	45 35·5	......	......	......	44 23·5 44 45·5	...... a,σ =	...... 120 39·67	43 47	......
ds	10 45·44	10 31·5 10 49	10 36·83	10 36 10 17·5	10 5	......	......	......	11 4	10 41·5	10 19			
as	55 31·67	55 36 55 13·5	55 10·33 (55 17·83)	......	......	......	......	......	......	55 27	......	......	55 46	
st	14 16·33	14 31 14 15½	......	14 5·75	......	......	13 51·5	14 10·5 (13 42)	......	14 11 25	14 31 (15 23)	......	14 2 13 43	......
at	69 48	69 40·5 69 31·5	......	......	......	......	......	......	......	69 38·25	......	......	69 29	69 54 69 49
$t\omega$	17 23·22	......	......	17 33·25	......	(17 8) 17 51	17 24·16	......	......	......	17 17	......		17 12 15 53
$s\omega$	31 39·55	......	......	31 39 31 42·5	......	......	31 15·67	......	......	......	......	......		......
ωx	9 6·14	......	......	......	......	9 19·8	9 10·4	......	......	......	9 33	......		9 7 10 16
tX	8 21·23													(10 22)
tx	26 29·36	......	......	......	26 31	27 11	......	26 2	......	......	......	......	26 18 26 56	26 19 26 9
$x\sigma$	24 11·4	......	......	......	24 23	24 11	......	......	22 49 (23 58)	......	23 55	......		23 46 (23 40
$\omega\sigma$	33 17·54	......	......	......	......	......	......	......	33 55	......	......	......		......
σi	11 43·67	......	......	......	11 33·33	......	......	......	......	......	11 38	11 45·33		......
a,i	47 47·57	......	......	...... (fd	...... 10 23·5)	47 54	......	......	47 31	......	......	47 40·25		......
a,x	83 42·64	......	......	......	......	......	......	......	......	......	......	......	82 50	83 1 π − 96 4

Calculated.		1	4	6	7	8	9	11	Strassburg.	Miller's measurements. C	A	B
	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′
$\beta(16\,16)$	3 53	......	4 14									
βz	8 6·35	......	8 27	......	8 7·75	8 0	7 59	......		8 7 *8 4	8 32	8 8
zk	4 49·65	......	......	4 22·67 (3 50·67)	4 30·5	......	......	......	......	......	......	4 30·5
βk	12 56	......	......	......	12 38·25	......	11 54	......		12 51		
kt	16 39·75	16 47·33	......	17 12	16 56·4	16 41 16 39	......	16 44		16 50	......	16 57
βt	29 35·75	......	29 34·5	......	29 34·66	16 16 29 21	......	......		29 41 *29 32	29 8	
$t\sigma,$	35 31·84	......	35 29	......	35 21·25 (35 26·7)	......	......	35 23·5		35 32	......	35 27·5
$\sigma g,$	23 5·25	......	23 4	......	...	......	......	23 12				
$tg,$	58 37·1	......	......	......	...	58 14 41 59	......	......		*57 29	58 38	
tz	21 29·4	......	......	21 24·67							20 36	
$c\beta$	43 52	......	43 51 (43 46)	......	......	43 48	......	......	43 48	43 57 $\pi -$ 43 58		
$\beta\omega$	27 0·5	......	27 1	......	27 0 27 1	......	......	......	......	27 1 26 55		
$c\omega$	70 52·5	......	70 52	70 45	......	70 27	......	......	70 53·67 70 49·25	70 52 $\pi -$ 70 49		
ωb	19 7·5	......	......	......	19 7 19 6·5							
$\omega\omega,$	38 15	......	......	......	38 13·5	......	......	38 17·5	......	38 19		
$c,(01\bar{5})?$	29 58·4	......	......	......		......	......	......	31 41			

* There seems to be a slip of the pen in denoting these measurements.

	Calculated.	3	4	6	8	9	10	11	12	Strassburg.
	° ′	° ′	° ′	° ′	° ′		° ′		° ′	° ′
$o\ (\overline{12}\ 1\ 12)$	10 35·16	......	......	10 31 (9 58) (10 49)	11 5					
$o\ (\bar{9}19)$	13 59·5	......	14 9·5 (?)	......	......		......		......	13 48
$o\ (\bar{8}18)$	15 39·6	18 34	16 2·5 (?)							
op	20 29·7	......	20 47·5	......	20 16 20 28					
$o\ (\overline{21}\ 4\ 21)$	23 8	......	......	23 3						
$o\pi$	24 9·5	......	......	......	......		......		24 44	
$o\ (\bar{9}29)$	26 30·5	......	......	......	......		......		26 28	
$o\gamma$	29 16·7	......	29 55·5 29 8	......	......		......		......	29 9·67 29 18 29 11·33
og	36 46·85	36 35·5	37 44·5 37 34	36 40	36 54		36 48·25 36 59 37 10		36 52 π − 36·37 π − 36·43	36 51 36 58 36 2·5
$o\chi$	48 16·4	......	......	......	......		......		......	48 10·75 48 11·4 48 18 48 3
$\chi\chi_{,}$	83 27·2	......	......	......	......		......		......	83 29·25
$gg_{,}$	106 26·3		105 15							
ob	90 0	......		......	......		89 57·5			
$\gamma\chi$	18 59·7	......	......	......	......		......		......	19 1·16 18 55
					On 1.		On 2.			
cg	57 50·5	......	......	57 55·75	......	......		......	57 57 π − 57 55	
gi	24 17·8	......	24 13	24 15·5	......	24 11·5		24 19		
$id_{,}$	27 16·1		27 10·5 27 1·5	27 23 (27 37)	......	27 6·5		27 9·5		
$gd_{,}$	51 33·9	51 40	......	......	......		......			
ci	82 8·3	(51 33)			...... 70 32					

	Calculated.	1	3	4		5	Miller. A	Miller. B
	° ′	° ′	° ′	° ′		° ′	° ′	° ′
$dd_,$	*96 27·2	96 26·33	96 8·67	97 8		96 45		
$ss_,$	77 30·5	77 37·4	77 59·33	77 38		78 6·5		
$ii_,$	91 2·16	......	......	90 59				
						on Strassburg		
$\sigma\sigma'$	70 46	......	......	70 53		70 48·67 π − 109 14·67		
		7	8					
$xx_,$	39 48·67	39 50·25	39 49	39 49·5		......	39 54	39 48
bx	19 54·33	19 56·83 19 53·16						19 51 19 57
($\bar{5}04$) g	37 49·2							
						11		
gx	35 6·8	......	35 3 34 37	34 56·5		34 51	34 52 35 6	
						Miller. C		
$x\sigma_,$	51 34·7	......	......	51 36				
ox	71 27·75	......	......	π − 71 31		71 30		
				15	D			
$t\beta_,$	68 50·7	68 46·33	......	68 51		......	69 2 69 10	
$tx_,$	39 15	39 13·33	39 4 38 48	39 12	39 20	39 5	39 12 39 23	......
$x\beta_,$	29 35·7	29 33	29 42·5 29 46·5	......	29 28	29 32	29 47 29 50	29 41
				11	6			
				° ′	° ′			
ωk	18 14·33	18 26·5	......	18 4 18 57	18 55	18 16	......	18 19
kh	12 37	12 22·5	......	......	12 17			
$h\omega$	30 51·33	30 47·75		......	......	......	......	30 45
$\omega t_,$	43 43·7	......	......	43 44 43 58	......	43 17		
		1						
c ($\bar{1}26$)	45 3·9	......	......	......	......	44 16 (?)		
cx	72 24·25	......	72 30	......	72 34	72 23		
$xk_,$	52 56	......	52 51	52 49	......	52 42		
$c_,k_,$	54 39·67	54 32·67	......	......	54 2	54 51		
cz	50 51·33	......	50 33	......	50 19	π − 50 31		

	Calculated. A	1	11	Miller.
	° ′	° ′		° ′
dk	35 8·5	35 9		
kξ	34 28·84	34 20·5		
dξ	69 32·16	69 29·5 69 34		
d,ξ	58 24·7	58 30·75		
ξ(104)	59 54	59 32		
ξs	60 2·9	60 15		
ξt	47 56	48 1·5		
ξt,	60 24·7	60 30		
ξξ,	98 5·7	98 13·25		
s,k	69 9	69 23		
d,k	75 15·4	75 22·5		
k(104)	53 34·67	53 30·5 53 28		
kk,	72 50·67	72 59		
mt	62 35·6	62 32 62 31		
tb	27 24·4	27 29 27 25		
tt,	54 48·8	54 54	54 49·5	
rr,	29 4			
		On 3.	On 5.	
gs	70 5·7	70 8	70 5	
gs,	53 42	53 43	53 12 53 55	53 34

	Calculated.	2	10
	° ′	° ′	° ′
cφ	71 46·75	71 42·7	
φψ	44 55·75	45 0	
oφ	68 52	68 55	
od	73 58·4	73 53·33 (74 0)	73 36 (73 52)
os	80 55	80 59 (81 6·5)	
ow	11 49	11 49·5 (11 56)	
oξ	42 37	42 42	
oξ,	137 23	137 20·5	
qq,	32 34	31 51·25	
cw	41 53	π − 41 57·5	
aw	58 5	57 57	
Nw	38 53·4	38 50	
hh,	97 30·85	......	
mk	58 2·4	......	
kx	24 26	......	
		7	Miller. B
xh	36 35·5	36 34·75	36 33
x,h	64 9·6	64 3	
sh	28 25·67	28 19	28 17

(3) *On Ansted's assertion, that the oldest rocks of Guernsey are to be found in the northern part of the island.* By Rev. E. Hill, M.A., F.G.S., Fellow and Tutor of St John's College.

Professor Ansted in his admirable monograph on the Channel Islands while describing generally the geology of Guernsey mentions incidentally that the oldest rocks of the island are to be found in the northern portion. He must have had ample opportunities for observing, as he tells us that he resided there four years. Being a trained and practised observer, there could be no reason beforehand to doubt the accuracy of his statement, and he has been followed by subsequent writers. As however in making this assertion he gives none of the evidence on which it is based, and as the statement has been repeated in a paper published in the proceedings of this Society, I think it may be well to put on record such evidence bearing on the question as in the course of several visits I have been able to collect.

The southern table-land of Guernsey consists, as the earliest observers noticed, of a mass of gneiss. This gneiss is extremely coarse, in some parts even more coarse I believe than that of Anglesey or the Hebrides, and to be matched only at Malvern, if at all. These rocks are the oldest in their several localities, and

nothing below them is known anywhere. Yet, coarse as is this Guernsey gneiss, it generally possesses a structure, whether we call it foliation or cleavage; and not unfrequently contains thin slaty partitions which can hardly be anything except beds. In some places the bedding can be ascertained with certainty, in others with considerable probability. The foliation can generally be made out, and agrees with the bedding where this can be seen. I have examined and registered these appearances along nearly the whole of the coast, and also at one or two points of the interior. The results are as follows.

The most northerly point where true gneiss appears on the east shore is in the rocks beneath Castle Cornet and the Castle breakwater. These are much shattered and veined and intersected by numerous dykes, so that the bedding is both variable and indistinct: it can however be seen at several places. At the end of the breakwater the dip is W. (steep): under Castle Cornet N.W. or N.N.W. (very steep), and at the middle of the pier, N.N.W. 60°. From the bathing-places I have noted dips both N.E. and N.W.: other signs show that the rocks are here much crushed and shattered. At the mouth of the tunnel there is seen a south dip, caused by a roll over of a few yards extent; and a similar roll may be seen on the shore in Petit Fort Bay, but the general dip seems N.N.W., about 45°. Going southwards it remains the same in direction, but under Kent Battery is very slight in amount, and continues to be only some 10° or 20° until within a few yards of Fermain Bay. Thus the gneiss from Fermain Bay northwards has a general northerly dip which though varying somewhat both in direction and magnitude if continued must make it finally plunge beneath whatever formations may be found beyond.

At Fermain Bay, or rather a few yards to its north, an abrupt change takes place. A quantity of dioritic rock seems to intrude, and the dips hitherto gentle with an E.W. strike alter suddenly to a vertical position and a strike N.S. This general character is maintained by them all the way to Jerbourg Point: the dip is generally W. and at one point as little as 60°, but is usually almost vertical. Sometimes it even leans over to the E. Similarly along the whole of the south coast, from Jerbourg to Pleinmont, whereever bay or cleft permits the coast to be reached, wherever quarry or crag enables the structure to be examined, there if a strike be seen that strike is almost always northerly, if bedding can be deciphered the dip is always steep. The following are my latest and most carefully taken notes: Moulin Huet; strike of foliation N.N.W. vertical, bending at end of bay to W. vertical: Saints Bay (here there are strong indications of bedding) strike N.N.W., dip 60° or 70° E.N.E. and, at the mouth of the bay, N., 80 W.: Icart Point, strike, N.N.W., dip steep E.N.E.: a quarter of a mile

further west, N. steep E.: Petit Bot Bay, N.N.W. 60 E.N.E.: Moye Point, N.W. very steep N.E.; stream half a mile west of Le Gouffre (not distinct) N.N.W. vertical: quarry on cliff at spot marked Prevoté Watch House, W.N.W. 60 or 70 S.S.W.: Creux Mahie N.N.W. 60 W.S.W.: Mont Herault, N.W. by N. 60 S.W. by W. From the interior I have only one trustworthy observation. This was made in a quarry south of the brickfield at the head of Les Talbots Road, and showed a strike N. by W. with a dip that varied in amount from 60° to 80°, but in direction was everywhere towards the west. Turning northwards along the coast of Rocquaine Bay we find well-marked slaty beds at Rocquaine Castle which strike N. and are nearly vertical. On Lihou there are strikes to N.N.E. and N.E. by E. with dips varying from 60 W to vertical. Onwards through Perelle Bay and Vazon Bay there are only imperfect indications, but so far as they go they point to strike N. and dip still vertical. After Vazon Bay the shore is occupied by the Cobo rock and the true gneiss reappears no more.

Now though the boundary of the gneiss cannot be traced across the interior of the island, yet some idea can be formed of its position. The blue rock of the North is seen in St Peter's itself on the East side of the island, and in some quarries near the Cobo Road on the West, and a sort of scarp runs across the country from near one of these points to near the other. These suggest that this boundary runs on the whole about East and West. This direction cuts right across the troughs and flexures indicated by the above observations of dip and strike. So far therefore no evidence is yet disclosed in support of the asserted superior position of the gneiss. The more natural conclusion would be that there is either a fault or an unconformity.

Next let us consider what evidence is afforded by the northern rocks themselves. Under the Esplanade such appearances of dip as can be conjured up are vertical. At Hogue à la Perre some white bands in the rock dip gently S. W. and further N. a strong joint structure dips gently W. Neither of these indications however are quite satisfactory. The former is neither persistent nor continuous: it seems to die out without any obvious reason, and is missing where one would expect to see it again. The latter occurs in a very homogeneous rock, is unaccompanied by any alternation of colours or texture, and seems analogous to cleavage rather than to stratification. Both are gentle, and quite compatible with a different dip within no great distance.

After this for more than three miles along the shore there are few appearances of any bedded structure, and those both unsatisfactory and inconsistent: some are S. and gentle, others N., and many vertical, but there are none in which confidence can be placed. Not till within the last mile of the coast, at Port

Norman Battery, do we find a slaty rock with a steep dip N. E. by N. Still further N. in a projecting bluff, some bands weather out dipping about 30 W. N. W., and at the very extremity of the island under Fort Doyle itself, a series about 20 feet thick of slaty beds dip 60° or 70° N. E. No dips can be obtained along the rest of the coast, and none of the inland quarries afford any appearances that can be trusted. Thus then the only really strong evidences of dip in the beds of northern area would carry them above the gneiss, and the rest are on the one hand in themselves weak, and on the other hand not inconsistent with this view.

In pages 76 and 77 of Vol. III. of the Society's proceedings it is mentioned that in a quarry on the W. side of Delancy Hill a bed of stratified quartzite dips to the N. W. This dip also would favour a superior position of the rocks in which it occurs. But it cannot be trusted. The quarry is now abandoned, and occupied by water, so that the bed cannot be reached. But observation shows that the main bed splits up on the right hand side into several thinner beds, which can be traced by the eye for many yards along the quarry-face, and which fork out and re-unite in a manner inconsistent with true bedding. One or two of these thin seams pass up to the summit of the cliff and, after a little trouble I succeeded in obtaining some specimens. These do undoubtedly possess a strong resemblance to a sandstone. But the entire absence of quartz from the general rock of the quarry was very suspicious; and the anastomozing arrangement of the veins was quite inconsistent with stratification. While I was revolving the possibilities of its being a fissure filled up, or a vein of segregation, a slide submitted to Professor Bonney, without any indication of its origin, was pronounced by him to be 'Probably igneous, allied to granite.' This exactly agrees with the whole behaviour of the rock. Those who are familiar with White Trap and other decomposing dykes will have no difficulty in comprehending the mistake.

We have in this case a good instance of what I have several times remarked before, that behaviour in the field is the surest evidence for the nature of a rock, and next to that, microscopic examination of a section. The appearance of hand-specimens is inferior in value to either.

Yet another line of argument may be taken up. It is clear that the gneiss has been subjected to immense transverse pressure. The generally vertical foliation, the abrupt flexures, the crushed condition of the beds, unite in testifying to this. Whatever force produced this must have acted simultaneously on all rocks then existing. If these northern rocks be, as Ansted calls them, older metamorphic rocks, this pressure would have affected them too, and could hardly fail to have developed in them a cleavage. Not

only is none such universal, but the numerous quarries which honeycomb their area, their tough and uniform texture, and above all, the quality of the paving-stones which they furnish are proofs that none such in general exists. These rocks are posterior to the foliation of the gneiss.

Thus then Ansted's assertion is not only unsupported by, but opposed to evidence. It is possible that he was misled by a supposed correlation with the rocks of Sark. But there seems no use in conjecturing the origin of a mistake. Besides the mistake in my opinion extends far wider and deeper. I believe these northern rocks of Guernsey to be as a whole igneous, and eruptive. The evidence of this will be given in a paper which is in process of preparation.

May 28, 1883.

Mr Glaisher, President, in the Chair. The following communications were made to the Society:

(1) *On the general occurrence of Tannins in the vegetable cell and a possible view of their physiological significance.* By W. Gardiner, B.A.

In the following paper I propose to consider as briefly as possible, the chemical nature of tannins, their occurrence, and what appears to be their physiological bearing in the general economy of the plant[1].

Under the head of tannins have been grouped a number of bodies which as far as our limited knowledge of them goes may be conveniently classed together, both on account of their general properties and reactions. Thus they have an astringent taste; a slightly acid character; they precipitate albumen and gelatin and give with ferric chloride either a blue black or a green colouration or precipitate. Most of them are glucosides being resolved by ferments or by heating with dilute acids into tannic, and finally into gallic acid, and glucose, and on fusion with potash yield in nearly every case protocatechuic acid and phloroglucin. They must consequently be regarded as aromatic glucosides.

Our knowledge as to the chemistry of the tannins has advanced but little since Sachs in his *Physiologie Végétale* of 1865 wrote "The physiological character of tannin can only be studied with any advantage when chemists have taught us something certain about its relations with other substances." All tannins were at

[1] The whole literature of tannin has been collected by Kutscher in his paper "Ueber die Verwendung der Gerbsaüre im Stoffwechsel der Pflanze." *Flora* 4. 1883.

first regarded as identical with the particular form occurring in galls, but later investigators have distinguished many varieties, such as Gallotannic, Quercitannic, Cinchona-tannic, Catechutannic, Moritannic and Caffetannic acids. Many of these are obviously related to one another, but since any thorough investigation has as yet been limited to gallotannic acid it is impossible to make a definite statement with regard to them.

According to their behaviour with Ferric chloride they can be well separated into two classes, viz. (1) those which give a blue black and (2) those which give a green precipitate. The blue black tannins and the green tannin of willow-bark were shown by Stenhouse[1] to be glucosides, while many of the others appear to yield phloroglucin (the isomer of pyrogallol) in place of glucose. It was Strecker[2] who pointed out that the tannin of oak-galls was in reality a glucoside, but it is to Schiff[3] that we are indebted for any really definite knowledge as to the formula and probable relations of gallotannic acid. He first prepared tannic acid free from sugar by the action of phosphorus oxychloride on tannin, and gave for its formula $C_{34}H_{28}O_{22}$, thus properly regarding it as a glucoside of tannic acid, and not as Strecker had done as a glucoside of gallic acid which necessitated for its formula $C_{27}H_{22}O_{17}$. According to Schiff tannin is decomposed by the action of ferments or dilute acids according to the following equations.

$$(a)\quad C_{34}H_{28}O_{22} + 2H_2O = \overset{\text{(glucose)}}{C_6H_{12}O_6} + \overset{\text{(digallic or tannic acid)}}{2C_{14}H_{10}O_9}$$

$$(b)\quad C_{14}H_{10}O_9 + H_2O = \overset{\text{(gallic acid)}}{2C_7H_6O_5}.$$

On boiling an aqueous solution of gallic acid to which a small quantity of arsenic acid is added, although the arsenic acid remains unaltered, the gallic acid loses water and tannic acid is produced.

Thus $$2C_7H_6O_5 - H_2O = C_{14}H_{10}O_9.$$

Thus tannic acid appears to be an anhydride of gallic acid. Furthermore by treating tannic acid with acetic anhydride Schiff obtained what was first supposed to be a tetracetyl, but which afterwards proved to be a pentacetyl tannic acid. There are thus five replaceable hydroxyls and we may regard tannic acid as an etherated anhydride of gallic acid—gallic acid being as we know trioxybenzoic acid. Thus

$$\overset{\text{2 (Gallic acid)}}{C_6H_2\left\{\begin{matrix} OH & HOOC \\ OH & HO \\ OH & HO \\ CO[OH & H]O \end{matrix}\right\}C_6H_2} - \overset{\text{(water)}}{H_2O} = \overset{\text{(Tannic acid)}}{C_6H_2\left\{\begin{matrix} OH & HOOC \\ OH & HO \\ OH & HO \\ CO & \text{———} O \end{matrix}\right\}C_6H_2}$$

[1] Stenhouse. See Watts, *Chem. Dic.* Vol. v. p. 660.
[2] Strecker. Watts, *Chem. Dic.* v. 660.
[3] Schiff. Deut, *Ges. Ber.* IV. 231, 967.

Thus as far as regards gallotannic acid its relations and structural formula have at least been stated, but with regard to the other forms of tannic acid we have at present no details.

Among the many reagents hitherto employed in botanical microchemistry as tests for tannin, none can be regarded as entirely satisfactory. Leaving out of the question such reagents as the soluble salts of barium, mercury, and antimony, whose precipitates in consequence of their white colour can only be recognized with great difficulty, if at all, there remains for consideration Schultz's solution (Chlor- Zinc- Iod-), Potassium bichromate, and the well known solution of Ferrous sulphate which has been allowed to oxidyze slightly, in order that some Ferric salt may be present. As to Chlor- Zinc- Iod-, which when diluted gives a violet colour with tannin, it must be regarded as highly unsatisfactory, not only because it is a very powerful reagent, but especially because it colours the cell wall and the starch grains, and from the latter reaction is liable to give very delusive results and to suggest some definite connection between tannin and starch, which is certainly not proved to exist from such a reaction and which considered from a chemical point of view is extremely improbable. Moreover Chlor- Zinc- Iod- gives no reactions with bodies very closely allied to starch, such as glucose.

Potassic bichromate[1] or chromic acid is an extremely useful reagent producing a well defined gelatinous precipitate of a reddish-yellow colour in the cells containing tannin. But with the nature of this precipitate we are by no means acquainted and from its colour it may as well be the reduced chromic oxide as a doubtful tannate of chromium; moreover knowing as we do how extremely easily chromic acid is reduced by such bodies as aldehydes, and even by alcohol, there is great probability that many of the precipitations produced by potassic bichromate or chromic acid do not point to tannin at all, but to some other reducing agent present in the cell sap. Another objection is the colour of the salt which of itself stains the tissues. It must however be frankly admitted that in the majority of instances chromic acid appears to be a perfectly satisfactory reagent and is nearly always confirmed by iron.

For the tannins which give a blueblack precipitate with the mixture of iron sulphates[2], this reagent is in every way trustworthy, in that here we have a distinct precipitation or colouration, the character of which admits of no doubt, but with regard to those tannins which give a green precipitate it may, as in the case of the Chromium, be reasonably doubted whether the pre-

[1] Sanio, *Bot. Zeit.* 3. 1863.

[2] See Sachs, "Einige neue Reactionsmethoden." *Sitz. d. k. Akad. in Wien.* XXXVI. 1859.

cipitate does not consist of the reduced oxide, and as such whether in every case it has been produced by tannin alone.

The following modification which I have lately discovered and adopted, appears to me to be free from many of these objections. Rose found that a solution of an alkaline molybdate gave a red colour with tannin. If however an excess of ammonic chloride be present, a voluminous yellow precipitate is produced. Moreover by means of this reagent tannin may be readily separated from digallic acid which latter only gives a red colour. In fact, whereas the compound of tannin and molybdenum (?) is insoluble in ammonic chloride, that with gallic acid is soluble. The reagent may be prepared by dissolving ammonic molybdate in a strong solution of ammonic chloride. The advantages I claim for this reagent are that it affords a means of easily separating the glucoside tannin from tannic (digallic) acid which could not satisfactorily be done by the iron method; that the solution is colourless; and that it gives this precipitate so far as I know with all forms of tannic acid, and is limited to tannins alone. The sections are simply cut and mounted in the solution.

Tannin always occurs in solution in the cell sap, and, as Schell[1] observes, such phenomena as the apparent occurrence of crystals of tannin or of tannin grains as described by Hartig[2], are due to the fact that the solid bodies present in the cell—be they crystals or starch grains—have become saturated with the tannin, and in consequence give a tannin reaction. In some instances the proportion of tannin may be so great as to considerably alter the refractive index of the sap of the containing cell, as occurs in the cells of the pulvinus of Mimosa and, as Schimper[3] has shown, in the gland cells of Sarracenia and Utricularia.

During the life of the cell the protoplasm is not affected by the tannic acid, but if the cell be killed, the dead protoplasm is attacked in the same way that albumen would be, and a definite precipitation or tanning of the protoplasm takes place. This explains why it is always possible to obtain the tannin-reaction with tissue which has been preserved in alcohol although, as we know, tannin is easily soluble in that liquid. This tanned protoplasm readily takes up any staining fluids, and becomes rapidly coloured either by extraneous dyes or by any colouring matter from neighbouring cells. Thus in badly preserved leaves of Ficus or Cerasus one can easily recognize the tannin cells which have now become stained brown. Such tannin cells present a granular-reticulate structure of the protoplasm, much resembling the structure of the precipitate produced by Chromic acid and tannin.

[1] Schell, *Annales Agronomiques.* IV. 1878.
[2] Hartig, "Das Gerbmehl." *Bot. Zeit.* VII. 1865.
[3] Schimper, *Bot. Zeit.* XL. 1882.

Tannin occurs very widely distributed in every part of the plant. Usually met with in isolated cells, it may be present in canals or passages, *e.g.* Angiopteris, Acorus. It is also found in laticiferous cells and vessels. It is produced abundantly in leaves, *e.g.* Cerasus, Rhododendron, Ficus, &c.; in stems, *e.g.* Humulus, Pinus, &c.; in roots, *e g.* Kramerea, Aspidium, and is stored up in large quantities in the bark, *e.g.* Quercus. Besides this it is found in most growing roots and shoots in embryos, *e.g.* Quercus, Borrago; in most germinating embryos in pulvini, e.g. Robinia, Mimosa and in the gland cells of the leaves of such insectivorous plants as Sarracenia and Utricularia[1]. Lastly it occurs in very large quantities in galls and other pathological tissues. As far as I am aware the endosperm does not contain tannin, and even in such seeds as Areca Catechu, where the tannin is of officinal importance, it is in the cells of the testa, and not of the endosperm that the tannin is present.

We are now in a position to consider the probable significance of tannin. It is to Sachs[2] that we owe "such knowledge as we possess," as to the probable function of this substance, and he it was who first made any important generalisations with regard to it. He showed that in germinating seeds tannin soon appeared in the cells even though absent before germination had commenced, or if as in Quercus—where the embryo had already attained large dimensions,—some tannin was already present; that the quantity rapidly increased. He also noticed that in growing organs tannin was rapidly produced, and remarked[3] that it was in fact formed in the immediate neighbourhood of those parts of the plant in which metabolism was most active. The general occurrence of tannin in leaves further illustrates this statement.

But what appears specially to confirm Sachs' views and to give the true clue to a satisfactory solution of the question of the physiological bearing of tannin is the occurrence of this substance in irritable and pathological tissue. Thus in pulvini, such as that of Mimosa pudica, very large quantities of tannin are present, and its formation appears to be in some way connected with the irritability of the protoplasm of these organs of movement. In the young and fully formed pulvini, at first none is present, but soon after the movements commence, tannin makes its appearance, and steadily increases in quantity until the death of the leaf, when the proportion of tannin in the cell sap is very considerable[4]. Moreover the amount of tannin present, appears to bear some direct relation to

[1] F. W Schimper, *Bot. Zeit.* XL. 1882.

[2] Sachs, *Physiologie Végétale*, 1882.

[3] See also Petzold, *Ueber die Vertheilung des Gerbstoffes in Holzgewächsen.* (Dissertation, Halle, 1876).

[4] Pfeffer, *Pflanzen physiologie.*

the sensitiveness of the organ in question. Thus in Robinia pseudacacia the movements executed by the main pulvinus are but few when compared with those of the pulvini of the leaflets, and one finds that the amount of tannin in the secondary pulvini greatly exceeds in proportion that present in the main pulvinus of the leaf.

Again, in the development of galls there is at first no tannin present, the gall tissue being simply a luxuriant growth in consequence of the stimulation produced by the presence of foreign bodies, viz. the egg and a small quantity of liquid—probably formic acid—injected by the insect into the tissue. Later on, owing to the irritation produced by the grub and the consequent stimulation of the protoplasm, tannin is formed in the cells and, as in the case of the spangle-gall, a quantity of starch also. Lastly, Schimper's researches on Sarracenia and Utricularia, in which he has shown that the gland cells are peculiarly rich in tannin; are of great value, since there again the protoplasm is extremely irritable and will respond to stimuli. All these facts appear to point to one and the same conclusion; that tannin is produced as a result of active metabolism, consequent either upon the rapid production of cells, as in the growing point, or consequent upon direct stimulation, as in pathological or irritable tissue.

As to the formation of tannin in the cell, it has been noticed that it usually occurs simultaneously with starch, and it has been put forward that starch goes to form tannin. At present however we have no evidence that this occurs. As Müller[1] pointed out, the simultaneous presence of starch and tannin in a cell does not prove that the latter is derived from the former, and our chemical evidence suggests that this is not the case. It is possible that the glucose part of tannin is formed from starch, but as far as regards tannic acid, it seems much more reasonable to suppose that it is formed directly from the protoplasm.

Finally, we have to consider the fate of tannin. The balance of evidence is certainly in favour of the view that tannin is a waste product and, as such, of no further use to the plant. In support of this view we have the fact of the occurrence of such large quantities of tannin in those parts of the plant which are thrown off, such as the leaves and the bark. In growing points tannin appears to be present in all the cells of the tissue[2], but it gradually becomes stored up in certain definite cells which are either isolated or in rows. In this respect it resembles such waste products as calcium oxalate. In a cutting of Cerasus lauro-cerasus, where the root had been formed and a quantity of shoots were being produced, I found that in the old leaves, which must have carried on the greater part

[1] Müller, Pringsheim's *Jahrbücher*, v. 1866.
[2] Kutscher, *loc. cit.*

of the metabolism, the quantity of tannin had very greatly increased, instead of decreasing. In galls and in pulvini there is in the same way a steady increase in the quantity of tannin. In seeds it is only when great growth of the embryo has taken place that tannin is produced[1] and, as in Areca, it is the testa and not the endosperm which contains it. Lastly, there is evidence both in animal and plant physiology which points to the fact that bodies containing benzene derivatives cannot be used up by the organism.

However, several observers have maintained that in reality tannin is of use in the general metabolism, and have stated that in growing points, for instance, the proportion of tannin gradually diminishes[2]. But it must be borne in mind that below the growing point great extension of the tissue is going on, and also that the tannin, from being a general cell content, now becomes stored up in definite cells, which moreover have undergone a very considerable increase in size. In fact, until there is further and very conclusive evidence on the subject, it may be reasonably doubted whether tannin is ever used up in the general cell metabolism.

With regard to the formation of resin from tannin[3], investigators have stated that the first step in its production is the formation of a glucoside, and that this glucoside is probably tannin. As Sachs showed in 1859, the cells from which the resin canals are formed are rich in tannin, as are the cells immediately surrounding the secreting layer. Still there is no direct evidence that tannin is the glucoside, any more than that coniferin[4], which by the action of dilute acid actually forms a resin, produces the resin of the conifers. It may be pointed out that the cells are very rich in starch also, and that there may be some connection between that body and resin. As Vines has suggested, when speaking of the resin grains observed by Hartig and Wiesner, "We have in the process of lignification the conversion of cellulose into an aromatic cellulide, could we not have in the formation of resin grains the conversion of starch into an aromatic glucoside?" However, at the present stage of the science we can only state that our knowledge of the subject is too incomplete to make any definite assertion.

Finally, in considering whether tannin is in any way concerned in the formation of the red or brown colour[5] produced in leaves by a long continued depression of temperature, we find that it is generally admitted that such is the case. Wigand and Schell also

[1] Sachs, *loc. cit.*

[2] See Schell and Kutscher, *loc. cit.*

[3] For the literature of this subject see Wiesner, *Sitz. d. k. Akad. in Wien.* LI. 1865. Franchimont, *Flora.* 1871. Hlasiwetz, *Sitz. d. k. Akad. in Wien.* LV. 1867. Schell, *loc. cit.*

[4] Von Höhnel, "Ueber das Xylophylin und das Coniferin." *Sitz. d. k. Akad. l. Abth.* 1877.

[5] See Wigand, *Bot. Zeit.* 1862. Hlasiwetz, *loc. cit.* Schell, *loc. cit.* Kraus.

believe that the red colour of very young leaves and of dying leaves in the autumn is also dependent on the presence of tannic acid. Doubtless also the phlobaphenes or colouring matters of the bark, *e.g.* the red amorphous colouring matter occurring in the periderm of the oak, are formed from tannic acid, since many tannic acids, such as Quercitannic and Cinchona tannic acid,—when boiled with dilute acids—yield red amorphous bodies resembling the phlobaphenes in every respect.

(2) *On the junction of the Root and Stem in the Monocotyledonous plant.* By M. C. Potter, B.A., St. Peter's College. With Plate XI.

In the root of both Monocotyledons and Dicotyledons we find the fibrovascular bundles arranged on the radial type, the transition from the radial type of the root to the collateral type of the stem has been investigated for Dicotyledons by Miss Goldsmith[1], who found that in the tigellum the Xylem and Phloëm of each collateral bundle separated from each other, the Phloëm bundles uniting in pairs, the Xylem bundles after rotation uniting in the centre, so that the protoxylem which is in the stem the most internal part of each Xylem bundle becomes the most external part in the root. As far as I am aware no one has traced the course of the fibrovascular bundles from the regular radial type in the root to the irregularly scattered collateral bundles in the stem of the Monocotyledon.

Professor Sachs[2] in his text-book of botany thus describes the germination in Monocotyledons.

"Germination begins either at once by the lengthening of the roots (their protrusion causing in Grasses the rupture of the root sheath which envelopes them) or, as is more commonly the case, the lower part of the Cotyledon lengthens and pushes the end of the root together with the plumule which is enveloped by the sheath of the Cotyledon out of the seed, while its upper part remains in the endosperm as an organ of absorption until the endosperm is consumed. In Grasses however the whole of the plumule projects from the seed, the scutellum only remaining behind in it, in order to convey to the embryo the reserve material of the seed."

There are then in the Monocotyledon two modes of germination; as examples of these I have taken *Zea Mais* as a type of the germination in the Gramineæ, and *Phœnix Dactylifera* to represent the more common mode of germination.

[1] *Beiträge zur Entwicklungsgeschichte der Fibrovasalmassen im Stengel und in der Hauptwurzel der Dicotyledonen*, Inaugural-Dissertation, Zürich, 1876.

[2] *Text-book of Botany*, 2nd English Edition, p. 619. *Bot. Zeit.* 1862 and 1863.

I. *Phœnix Dactylifera.*

In *Phœnix Dactylifera* there are generally in the root nine Xylem bundles alternating with these nine Phloëm bundles; (see figure II. where the Xylem bundles are shaded;) the central part of an older root is composed of thick walled cells. Surrounding this as a central axis we find the pericambium, and external to this the endodermis. In older roots the endodermis is composed of thick-walled cells and it is first seen where the fibrovascular bundles of the root branch into the Cotyledons (figure III. where *E* is the endodermis). The fundamental tissue lying outside the endodermis contains numerous strands of sclerenchyma (*B* fig. II.) arranged more or less in rings and large intercellular spaces.

As we pass upwards from the typical structure of the root towards the apex of the stem we find the Phloëm bundles increasing in size, assuming irregular V shaped forms, uniting and anastomosing with each other; at the same time the Xylem bundle has increased in size more especially at its external part where the protoxylem is situated. As we approach the base of the Cotyledon the Phloëm bundles branch, sending branches into the Cotyledon. The Xylem bundles now divide; each branch from a Xylem bundle unites with one from a Phloëm bundle, and these constituting a collateral bundle pass outwards into the Cotyledon and unite with a strand of sclerenchyma which passes from the root up into the Cotyledon. Thus in the Cotyledon there are as many fibrovascular bundles arranged at nearly equal distances as there were bundles of Xylem or Phloëm in the root, the Xylem being the most internal, the Phloëm next, and the sclerenchyma the most external constituent.

Above the point of insertion of the Cotyledon with the stem we find collateral bundles irregularly scattered, uniting with each other in pairs and sending off branches into the leaves which enclose the very short growing point of the stem (Fig. V. and VI.).

II. *Zea Mais.*

The stem of the Grasses is characterized by the presence of nodes on the stem where the leaves are given off. Here the fibrovascular bundles anastomose freely with each other, often changing from one side of the stem to the other, the anastomosis being so complete that it is often impossible to trace the separate constituents of each bundle or the bundles themselves through the node.

The germination of *Zea Mais* is represented in Fig. VII. From the seed *S* the edges of the scutellum *Sc* are seen ruptured by the protrusion of the root *R* and the plumule. *C* is the so-called Cotyledon, *A* the node from which it springs. At *B* is another node, from which the fibrovascular bundles are given off to the

scutellum. The arrangement of the fibrovascular bundles in the root is on the radial type, as in *Phœnix Dactylifera*, only there are many more bundles of Xylem and Phloëm in the root of *Zea Mais* than in that of *Phœnix Dactylifera*. On the outside of the fibrovascular cylinder in older roots there is an endodermis of one or two layers of sclerenchymatous cells. As we approach the node *B*, passing upwards, the bundles become irregular in form and show signs of rotation as they enter the anastomosis of the node, but it is impossible to follow the course of each bundle through the node. Between the nodes *A* and *B* the arrangement of the bundles is irregular. In Fig. VIII., which is a drawing of a section cut between the nodes *A* and *B*, spiral vessels are seen at *S* irregularly placed inside a more or less continuous ring of Xylem, often opposite the Phloëm bundles *Ph*, which are external to the ring of Xylem. This arrangement continues as far as the node *A*, where the bundles again undergo an anastomosis, and above this we find the typical structure of the grass stem, viz. irregularly scattered fibrovascular bundles arranged on the collateral type and not enclosed by an endodermis.

A comparison between the passage from root to stem in Monocotyledons and Dicotyledons may now be made.

In the procambium of the root the protoxylem or spiral vessels, and the protophloëm or bast fibres are first differentiated, the differentiation in each bundle proceeding from without inwards and thus the separate Xylem and Phloëm bundles are produced. In the stem each bundle consists of Xylem and Phloëm. The protoxylem is first differentiated at the most internal part of each bundle, and the differentiation proceeds from within outwards, while the protophloëm is first differentiated at the most external part of each bundle, and the differentiation proceeds from without inwards.

Thus in the stem the protoxylem is the most internal part of each collateral bundle, and in the root the most external part of each bundle of Xylem, and in passing from the root to the stem there is necessarily a rotation of the secondary Xylem round the protoxylem, or a line drawn through the protoxylem in the direction of the differentiation of the constituents of each Xylem bundle would in passing from root to stem turn through two right angles. Since in the root each bundle consists of Xylem or Phloëm only, and in the stem of both Xylem and Phloëm, it follows that in the transition from stem to root the Xylem and Phloëm of each fibrovascular bundle of the stem must separate from each other. The Phloëm bundles unite in pairs, the Xylem bundles rotate and unite in the centre of the root.

In the Dicotyledon the transformation from the arrangement of the bundles in the stem to that of the root generally takes place in

the tigellum. When the Cotyledons of the seedling are opposite, the collateral bundles divide and descend through the hypocotyledonary portion of the stem to the root, the Xylem separating from the Phloëm. The Phloëm bundles now unite in pairs, each bundle uniting with its neighbour and forming the Phloëm bundles of the root: the Xylem bundles rotate, so that the protoxylem becomes the most external part of each Xylem bundle and the secondary Xylem the most internal, and then uniting in pairs each one with the nearest, the protoxylem uniting with protoxylem, and the secondary Xylem with secondary Xylem. The Xylem bundles so formed coalesce with each other to constitute the central Xylem of the root.

In the case above described the number of Xylem and Phloëm bundles in the root is even, but in those cases in which the Cotyledons are not placed opposite each other, their divergence being $\frac{1}{3}$ or $\frac{2}{5}$, the number of Xylem or Phloëm bundles is odd, being 3 or 5 respectively. The odd bundle situated on the opposite side of the stem to the Cotyledons does not come from the Cotyledons but from the lowest leaf of the stem. The bundles from the Cotyledons continue in the manner described above, the odd bundle coalescing with the others to form the axial fibrovascular cylinder.

In the Monocotyledon the root arrangement of the bundles continues nearly as far as the point of insertion of the Cotyledon in *Phœnix Dactylifera* or of the scutellum in *Zea Mais*. At this point in *Phœnix Dactylifera* each Xylem and Phloëm bundle gives off a radial branch to the Cotyledon, and above this the Xylem and Phloëm bundles unite in pairs to form collateral bundles. In the root each Xylem bundle has the protoxylem at its exterior. The number of constituents of this protoxylem increases before the branch is given off to the Cotyledon, and generally the rotation of each Xylem bundle begins after the bundle has branched, but in some cases it appears to have commenced before the branch is given off. This rotation is completed before the Xylem and Phloëm unite to form the collateral bundles of the growing apex of the stem. In *Zea Mais*, at the insertion of the scutellum there is a node where the bundles anastomose. Here I have not been able to trace the course of each bundle. The bundles depart from the regular type of the root just before this node and the Xylem commences to rotate; above the node we find the protoxylem the most internal part of a more or less continuous ring of Xylem showing that the rotation is complete, but that the pairing off of the Xylem and Phloëm is not finished. This arrangement continues as far as the next node, where again there is an anastomosis, and above this we find the Xylem and Phloëm united in pairs to form collateral bundles.

Description of Figures.

Fig. I. Represents the germination of *Phœnix Dactylifera.* *C* is the Cotyledons growing out from the seed *s* and enclosing the first leaves. *R* the root.

Fig. II. The normal structure of the root. *A* the epidermis, *E* the endodermis, *Xy* the Xylem, *Py* Phloëm, *B* strands of sclerenchyma in the ground tissue, a section through the plane II. II. in Fig. VI.

Fig. III. A section through the plane III. III. in Fig. VI. The Phloëm has united and increased in size. A branch is being given off at *D* to the Cotyledon. The Xylem masses are irregular and beginning to branch.

Fig. IV. A section through the plane IV. IV. in Fig. VI. The Xylem is giving off branches to the Cotyledon. The Xylem branches turn off nearly at right angles, as in Fig. VI. At their ends are seen the Phloëm bundles.

Fig. V. A section through the plane V. V. of Fig. VI., showing the irregularly scattered bundles in the centre and the bundles in the Cotyledon *F*.

Fig. VI. A diagramatic longitudinal section. *CC* section of the Cotyledon. *LL* section of the first leaf. The bundle is drawn in the plane of the paper, but in the Monocotyledonous plant the bundles do not remain in one plane, but take a kind of spiral course.

Fig. VII. The germination of *Zea Mais.* *S* the seed, *Sc* the edges of the ruptured scutellum, *C* the Cotyledon, *R* the root, *AA* the node from which the Cotyledon is given off, *BB* that from which the fibro-vascular bundles are given to the scutellum.

Fig. VIII. Transverse section taken between the nodes *AA* and *BB*, *Ph* the Phloëm bundles, *S* spiral vessels, *Xy* Xylem vessels.

(3) *Some Observations on the Swelling of Starch Grains.* By WILLIAM HILLHOUSE, B.A., Professor of Botany in the Mason Science College, Birmingham. With Plate XII.

Certain peculiar appearances which had come under my notice as the result of the continued action of Chlorzinc Iodine (Schultze's solution) on starch grains in the winter tissues of plants, prompted me to undertake a few separate experiments with the reagent[1],

[1] The following is the method, based on that of Radlkofer, in which I prepare this reagent:—Pure granulated Zinc is dissolved in Hydrochloric Acid at an ordinary temperature; the solution is evaporated, at a temperature of about 70° or 80° C., and under contact with metallic zinc, to a syrup which does not get muddy on addition of much water, and has the sp. gr. 2·0. This syrup is poured off and diluted with water to sp. gr. 1·8, that is, 12 parts of water are added to every hundred parts of the zinc chloride solution. In 100 parts of the resulting fluid dissolve at a gentle heat six parts of Potassium iodide, and then dissolve in the

experiments which I mainly carried on in the Botanical Institute at Bonn. For this purpose I used dry old arrowroot starch. The results of the investigation appear to be of some interest and importance, as bearing on the physical constitution of the starch grain, and the various theories of its growth.

As my original investigation was undertaken in connection with the presence of Tannin in the same cells with the starch grains, I had allowed some of this dry arrowroot starch to lie for a few days in a weak aqueous solution of tannin, and tested these with the reagent, at the same time testing other grains which I had merely damped with absolute alcohol. Some of the slides thus treated happened to lie upon my table, and to be examined the next day, when I was surprised to find that in one of these preparations the starch grains were mostly swollen in such a way as to preserve distinct lamination (see Fig. 8). Whether the alcohol or tannin grains had so swollen I could not say, so I repeated the experiments with numerous cases of both, carefully watching the progress of the phenomena. It is unnecessary to give details of these experiments, the results of which, especially with the tannin specimens, varied considerably. I have therefore collected into a succinct form the various facts observed.

Grains which had lain for three days in tannin[1] solution showed very few signs of stratification. The nucleus (hilum) was rarely visible, though its position was not infrequently indicated, as with dry grains, by a dark cleft,—dark, from the presence of air or other gas, which in the subsequent solution of the grain came out in the form of one or more bubbles, and immediately disappeared, by solution, in the surrounding reagent.

One or two minutes after the application of Chlorzinc Iodine to the grains (a little of the tannin solution remaining on the slide, and therefore somewhat diluting the reagent) here and there a grain can be seen to swell into an irregular mass, and turn of a bluish colour; most of the grains are however totally unaffected, but in the course of a few minutes here and there one shows, without swelling, a blue tint, varying in intensity in the same grain from dark blue at one (the narrow) end, gradually getting paler towards the other (and broader), while this latter appears covered with irregular dark-blue spots, on a pale reddish-blue ground. In the course of a few more minutes some other grains have swollen and become blue; but it is evident

whole as much Iodine as it will take up. The solution will now have the consistence of concentrated sulphuric acid, is perfectly clear, of a bright golden-brown colour, slowly becoming somewhat darker on exposure to light. It can be brought to various degrees of dilution, as its action varies according to its strength. It is best kept in the dark.

[1] The tannin is of course non-essential. The results are the same with distilled water.

that in the meantime many grains have swollen, and imperceptibly disappeared, without coloration; while others one can observe to swell, split, and disappear, the outer layer of each sector of the grain dissolving last, being visible to the end as an apparent membrane, of which the free edges (*ends* in optical section) are carried back by the swelling internal mass;—this swelling and solution likewise without coloration. The same process continues for some considerable time (perhaps an hour) the chief signs of staining being merely a violet-red rim to many grains. But it is noticeable that many other grains which have shown none of the common signs of swelling are perceptibly smaller, and that, without any of the processes above indicated, the pulpy mass around them has enlarged. Each of these solid reduced grains preserves its shape and distinct outline, but the surrounding pulpy mass takes a faintly violet tinge. The solid remnant still shows sometimes nucleus and stratification. In twenty-four hours the preparation forms a homogeneous mass of dissolved starch, with here and there only, a solid remnant. In some preparations, however, after this period a few grains show the peculiar lamination illustrated in Fig. 8, and which will be hereafter more specially dealt with.

The fact that we had here evidently to do with not one, but several distinct methods of swelling, made it clear that nothing was to be done with merely periodical inspection. I therefore kept a series of preparations each under continuous observation for periods up to about three-quarters of an hour in length, with resting intervals between these periods. In this manner the following five methods of swelling could be distinguished:—

1. The grain swells *en masse*, and resolves itself, without rupture, into a mass of "fovilla."

2. The grain swells *en masse*, then ruptures, and passes into fovilla from the point of rupture.

3. The grain swells from the outside to a certain depth, the swollen part gradually dissolving off from the outer side, while the swelling penetrates further inwards. A solid remnant of the grain is therefore left, gradually and regularly diminishing by the progress inwards of the swelling.

4. The grain swells from the outside to a certain depth, the outer swollen part ruptures and passes into fovilla; the remnant again swells externally as before. Two concentric swelling rings are sometimes visible.

5. The swelling takes place by laminæ, apparently equivalent to the sheaths or "complexes of lamellæ" of the grain. The whole resulting swollen mass of the grain is laminated.

Which of these methods takes place in a particular preparation is apparently uncertain and irregular. Sometimes nearly all the

grains swell in one way. This is especially the case with alcohol grains, swelling, as will shortly be described, in method 5. In one carefully watched case with *dry* grains treated with the reagent, as the earliest result of the action of the reagent 1 took place for some time alone, then appeared 2, and these two methods went on together. Later occurred here and there a case of 3. Last of all were a few grains only which swelled in the method 5. In this preparation no case of 4 was observed.

The method 4 is itself specially interesting. It is very infrequent, but I followed with great care, and drew at short intervals, several cases of it. Outwardly it greatly resembles 3, with which, in observation not of the closest and most continuous kind, it would not unnaturally be confounded. The grain swells to a certain depth, the swollen part being distinguished from the rest by its refractive index, and a clear limiting line which cuts it off internally. This limiting line slowly moves inwards. Another less turgid ring may be sometimes seen inside this one. The outer boundary of the grain is a strongly defined dark line. Suddenly this zone ruptures at some point, and from that point dissolves rapidly away in both (all) directions round the grain, till it has disappeared. The residual grain has the same dark boundary line and resisting limiting layer as before, and its boundary appears dark immediately on the solution of the outer layer from that part, and therefore progressively round the grain (Figs. 1 and 2). In this method of solution slight signs of lamination are usually visible in the surrounding fovilla (Fig. 1).

Leaving for the present any discussion of these phenomena, let us consider those observable in the case of starch which was first merely damped with alcohol. The grains themselves closely resemble those of the tannin preparations; stratification is rarely visible.

On addition of Chlorzinc Iodine no change is immediately visible, but in the course of a minute or two the grains begin to get feebly yellowish-red round their margins, and gradually, in a few minutes, the whole grain becomes a very pale yellow-red[1]. Here and there a grain gradually changes this colour to a rusty red; not uniformly, but with irregular patches of paler colour. The nucleus sometimes becomes more clearly visible, as also does the stratification in its immediate vicinity. With careful focussing so as to get an optical section of some one particular grain, the outer layer appears usually of a deeper rust colour, and a *double outline* to the grain can in some be clearly seen (Fig. 3). Sometimes for several hours there is no further change in the state of the preparation, excepting what is involved in the deepening of the colour, and, with individual grains, its gradual modification into a more

[1] Cf. Nägeli u. Schwendener, *Das Mikroskop*, Zweite Aufl. S. 514.

violet red tint. In such cases the swelling of the grains, and formation of laminated fovilla, proceeds too slowly to allow one to observe anything but the ultimate effects; but sometimes grains can be noticed of which the swelling is more active. Such a case is figured, at short intervals, in Figs. 4—7. The grain can be seen to have almost imperceptibly exfoliated, as it were, layer after layer from a gradually diminishing solid remnant, which preserves in general a position in the mass of fovilla analogous to that of the nucleus in the normal grain. Usually, though not very markedly in the examples figured, the lamination is much more pronounced, and the laminæ apparently more numerous, in the direction of the longest radius of the grain, that is, the side on which the normal lamellæ are best developed. Externally each lamina is distinctly limited, while in optical section it often shows its two limits by a double line. The smaller and rounder grains usually show the process first.

After twenty-four to forty-eight hours the whole of the grains in a preparation are swollen, the whole mass of fovilla corresponding with each grain laminated, and more or less corrugated. In cases where the solid remnant of the grain has not entirely disappeared the outer portion of this frequently shows signs of swelling similar, but on a more delicate scale, to that described in methods 3 and 4. The mass of fovilla has taken a purplish-red tint.

The length of time during which the laminated swollen grains retain these appearances, while still remaining surrounded by the reagent, varies considerably, sometimes extending to 4 or 6 weeks. Ultimately they are entirely dissolved, forming a mass of colourless fluid fovilla, if such a term can be applicable.

Now what is the bearing of these phenomena on our knowledge of the physical and anatomical constitution of the starch grain? Let us premise that variability in the action of the reagent is of no special importance, beyond telling us, what every physiologist knows already, that very slight variations in the conditions may induce considerable differences in the results. If we assume that starch grains are formed in any *one* way, we have equally to face variable appearances in the mature grains, as well as varying responses to the action of reagents. In order then to answer the above question we will first eliminate the phenomena which give us no *positive* information. Thus swelling by method 1 tells us nothing but what, having regard to variations in conditions, may be referred to method 2. In this case (2) the grain ruptures. This implies that the outer layer of the starch grain is resistant. On rupture the free edges curl outwardly,—the expression of the tension to which the layer is subjected, and by which its rupture is brought about. Further than this the case gives us nothing but negative results.

Method 3 again gives no information distinguishable from 1 but what may be referred to method 4. In this method of swelling (4) we have an additional phenomenon. We have here, also, an outermost resisting layer which overlies an unresisting layer of some thickness. The latter becomes turgid, overcomes the resistance of the outer layer, ruptures it, the whole is dissolved off, but only to expose to the action of the reagent another resisting layer similar to the first, followed by a similar soft band, yet another resisting layer, and so on. From this it follows, if my deduction be true, that the starch grain is composed of a series of layers each bounded outwardly by what is sometimes a firm "limiting membrane," of some modified organisation, which for some time resists the action of what is, for its subjacent layer, an easy solvent.

What now do we learn from method 5? Here we have clearly another case, showing the "layering" of the grain, but, showing further that, under certain circumstances, each separate layer with its outer limiting membrane can swell, become apparently detached from its subjacent layer, pass outwards in position, but yet maintaining its own continuity.

This brings up a highly important question to solve, namely to what extent the laminæ in the swollen grain are independent of one another, if they are so at all. In order to solve this problem there is only one method open to us—the application of pressure, with the intent to separate one from another these different layers, if they be so separable. Bearing in mind that we have here to do with soft laminæ, exceedingly delicate, and in a semi-glutinous state, and therefore tending to be mutually adherent, and that the only basis we have to work upon is the fact, deduced from 4, that the outer part of each lamina is more resistant than the material within and without it; bearing this in mind, I should not have been surprised had pressure failed to give any ascertainable results. This was, however, by no means the case. While the great majority of the swollen grains did what was expected of them, that is, flattened, and then went into a disorganised heap, here and there all over the field could be seen delicate fragments of the outer laminæ, often of considerable peripheral extent (Figs. 9 and 10), sometimes flattened out, sometimes folded, but sometimes preserving well-nigh intact the shape they bore while on the grain from which they had been squeezed; while in other cases the grains themselves could be seen with their envelopes torn, opened out, and separated in every conceivable manner. Examples of this will be seen in Figs. 11, 12, and 13; Fig. 13 representing a case of a peculiarly striking nature.

These facts, I think, justify us in concluding that the envelopes are anatomically distinct from one another.

Let us now bring these results to bear upon our interpretation

of the structure of the starch grain. Divesting ourselves of all bias derived from pre-conceived notions in favour of one or another of the theories which have been promulgated, what are the simple conclusions which we should derive from a consideration of evidence such as the above? They appear to me to be these:—That a starch grain is composed of a series of (in a broad sense) concentric layers, each bounded externally by a "limiting membrane" of the same substance as the rest of the grain, soluble in the same reagent, but often differing in its physical property of greater resistance, and that the outermost layer of the grain is bounded exteriorily by the most resistant of all these limiting membranes.

If our attention were drawn to a starch grain in its perfect state, and we saw the series of lines which pass around the excentrically placed centre of the grain, we should not, I think, hesitate much in saying that, if those lines mean anything at all in the anatomical structure of the grain, they have some relations to the lamination shewn above to exist in the swollen grain. But, counting the number of these lines existing in the starch grain, and comparing it with that of the laminæ in the swollen grain, a great disparity will be seen generally to exist. The latter are outnumbered two or three or more to one. Return to the starch grain, and it is seen that some of the lines are very delicate, while others are strongly marked, and indeed, if the grain be at all "fresh," these latter will be the only ones visible. Hence we should probably say that in the action of the reagent the delicate layers, (and in some cases all the layers) are, as it were, merged, and that the envelopes in the swollen grain correspond with the groups of layers in the grain itself, or, to introduce here the terminology of Strasburger[1], correspond with the "sheaths" (*Schichten*) or "complexes of lamellae" (*Lamellen-complexe*) of the unswollen grain, and not with the primary layers, or lamellæ; and that what I have here described as the "limiting membrane" of the envelope corresponds with the "Grenzhäutchen" hypothecated by the same author in the modern revival of the old theory of growth by apposition. It must however be noted that Strasburger applies this word to what he calls the denser *inner* edge of a complex of lamellæ, while I apply "limiting membrane" to the denser *outer* layer of a sheath, for such I take it to be.

The theory of growth by apposition alone appears to me capable of explaining these phenomena in the swelling of starch grains, phenomena which are quite inconsistent with the Nägelian theory of the physico-anatomical structure of the grain. The "limiting membrane" probably arises from a physical change which has taken place in the outer part of the grain during a temporary halt

[1] *Ueber den Bau und das Wachsthum der Zellhäute*, 1882, S. 6.

in its growth; while if such halts were but slight the grain would have a more uniform character, and its solution would likewise be more uniform.

It is perhaps of interest here to note, as it has a bearing on the conclusions from this investigation, though none on the investigation itself, that some old "tous-les-mois" starch (*Canna indica*) which I placed in absolute alcohol nearly 5 years ago, the alcohol being since once or twice changed, has not even yet lost its appearance of stratification, by loss of water. The doubts early induced by this were of course at the time rank heresy, though to my own mind they were fully substantiated by observations made (at Kew) about two years later, on the formation of compound starch grains in the "reservoir cells" of the cortical parenchyma of various species of *Rhipsalis*.

Explanation of the Figures.

(All Figures × about 600.)

Fig. 1 and 1*. Solution of the starch grain by successive zones, each with rupture of the outer "limiting membrane." Fig. 1 after 1 hour's action of Chlorzinc Iodine. Between this and Fig. 1* an interval of 40 minutes continuous observation. Slight lamination of the fovilla shewn.

Fig. 2. Another case. Between (1) and (2) was an interval of 10 minutes; between (2) and (3) of 25 minutes. In (3) the air has disappeared from the irregular cleft in the grain; and an extremely thin new zone of swelling is shewn.

Fig. 3. Grain shewing solid remnant after 48 hours in Chlorzinc Iodine. Outer fovilla shows signs of lamination.

Fig. 4—7. Progress of solution of a grain, and formation of laminated fovilla, drawn at intervals of about 5 minutes.

Fig. 8. Laminated fovilla, resulting from the solution of an isolated grain; drawn after 24 hours in Chlorzinc Iodine. The depth of shading indicates depth of coloration.

Fig. 9—10. Fragments of laminæ separated by pressure.

Fig. 11—13. Laminated fovilla, resulting from the swelling of single grains, and subjected to pressure, after 48 hours in Chlorzinc Iodine.

(4) *Crystallographic notes. By* R. H. SOLLY.

Euchroite $Cu_3As_2O_8 + H_2CuO_2 + 6aq.$

When registering the specimens of Euchroite in the University collection I noticed that some of the crystals exhibited the faces (011) (012) with the common combination.

As I am unable to find these faces recorded in any work, I therefore give the following angles I obtained with their calculated values:

	Found	Calculated
(011) (001)	59°.26′	59°.36′
(011) (110)	42.30	42.33½
(011) (101)	69.22	69.26
(012) (001)	39.40	40.26

The faces (001) and (110) were uneven and striated. The face (012) narrow and dull.

Hoping to find better crystals I examined the specimens at the British Museum and the Royal School of Mines, the latter place now contains the very valuable Ludlam Collection, but I was disappointed as neither collections possess crystals exhibiting this combination.

The figure in Plate X. represents these new forms drawn in fair proportion to the other faces.

Orthoclase.

When demonstrating last term on Orthoclase and making projections and drawings of some of the principal crystals for the use of the students, I noticed on an Elba specimen a well developed rough face (10 8 1) in the zone [110 201]. The roughness of the plane made it impossible to read with any degree of accuracy. So I endeavoured by searching through other collections to find a crystal with this face evenly developed.

The Royal School of Mines does not possess one. Prof. Warrington Smyth's valuable collection, which is very rich in Elba specimens, does not contain one exhibiting this face, and this was also the case in other private collections which I had the opportunity of examining.

In the British Museum I could only find one crystal exhibiting this face, which was kindly placed for me on one of Fuess's goniometers; though the plane was well developed (on account of the same roughness) it gave no better results than the specimen in the University collection.

	Found	Calculated
(110) (10 8 1)	6°.30′—7°.30′	7°.2′

This new plane is represented in Plate X. with the usual Elba combination.

Gahnite $(ZnO + (Al_2)O_3)$.

Three or more crystals twinned together according to different {111} faces of the Tesseral system have long been known, and are well described by Von Struver in Groth's *Zeitschrift*, Vol. II. p. 480 in the case of Spinel.

In the University collection there is a double twin consisting of three perfect {111} faces each being equally developed, similar to figure I. in Von Struver's paper. I can find no record of such a combination of crystals having been observed in Gahnite before, and as I could not find one in any of the many collections I examined, it may be considered a rare and interesting specimen.

Von Struver in his work has considered the first crystal A as the simple crystal and has then referred the other two crystals B and C to it. This method gives to the {111} faces of C the high indices of (11 11 1) and (13 5 7).

But if we refer the two crystals A and C to B as the simple crystal we get a simple symmetrical stereographic projection with the {111} planes as {511}.

I first observed such twins when working on the Zinc blendes in the British Museum by perceiving one of the cubic planes in an abnormal position, which would necessitate indices (744).

That the face was that of the cube was proved by its physical character, and it was necessary therefore to consider its abnormal position to be due to double twinning about a central crystal which in this case was so thin or so buried as to be invisible.

Thus when the physical character of the faces is such as to render it certain that they belong to a definite form, we may suspect that they are faces on a portion of the individual which owe their peculiar position to twinning about a central crystal, and we are thus able in the case of blende to perceive the existence of twinning even when the nucleus is so minute or so completely enveloped as to be otherwise imperceptible.

(5) *On the Refraction observed at Sunset,* (1) *near Trinidad,* (2) *near Rio.* By Dr PEARSON.

1. On Jan. 2, 1883, I was in a ship anchored at Port-of-Spain, Trinidad: Lat. 10°·39′. N. The horizon being quite clear towards the west, I observed that the exact interval between the first and second limbs of the Sun passing the horizon was two minutes thirteen seconds. I had another equally favourable view of sunset near Rio Janeiro, on March 28, in Lat. 18°·7′. S. when

the interval was two minutes fifteen seconds. But on February 7, in Lat. 28°· 5′. S. off the West coast of South America, the Sun setting behind a low bank of clouds reaching perhaps 2′ above the apparent horizon, the interval was two minutes thirty seconds: and lastly, March 11, Lat. 23°· 23′. S. near Rio Janeiro, the interval was two minutes thirty-one seconds. I cannot however assign so much value to these two observations as to the two others: in the third case on account of the uncertainty of the horizon, and in the last from the fact of my being in my cabin at the time, with the port not more than six or eight feet above the water, the eye on deck being on an average at the height of 20 feet, far more suitable to give a clear horizon.

Only in the first case was I able to test the absolute value of the horizontal refraction: in the other examples the uncertainty of my geographical position deprives any calculations of real value: I therefore defer for the moment to consider this point. But if we compare the Sun's diameter with the arc which the Sun's centre would traverse during the interval between the Sun's first and second limb passing the horizon, we get,

	1st case.	2nd case.	3rd case.	4th case.
Arc traversed:	30′· 0″.	32′· 40″.	31′· 30″.	34′· 45″.
Sun's diam^r.	32′· 36″.	32′· 6″.	32′· 30″.	32′· 15″.
Difference.	2′· 36″.	34″ (–).	1′· 0″.	2′· 30″(–).

In a communication to this Society made March 8, 1880, (*Proc.* Vol. III. 358) describing some observations made by me with a theodolite in Norway on the Sun near the horizon I have said that the "observations with the theodolite gave uniformly the position of the Sun lower on the lower limb than on the higher one": in which case it is clear that the error would have been less if the amount deducted for refraction had been that due to the position of the Sun's centre and not of the limb observed. But in the cases which I am now discussing, it would seem as if the Sun actually disappeared in a form differing on each occasion: the retaining power, if I may so express myself, of the increase in refraction between a point at a distance above the horizon equal to the Sun's diameter, and the horizon itself, being variable.

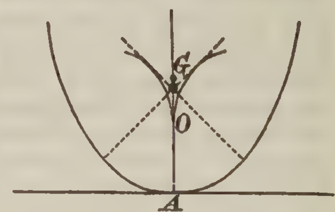
I desire to draw attention to this point, because I am not aware that the effect of the Sun on Refraction has ever been considered separately: and some observations of the recent Transit of Venus may perhaps have been made on the Sun when so near the horizon that this point may become worth consideration. I have no more to say on what seems to me a very curious fact: except that at Trinidad the actual refraction seemed to me pretty nearly what might be expected. Allowing for a *dip* on the ship's deck of about 4′, or a zenith distance of 90°· 4′. Bar. 30.0. Th. 84°. F.

with the simple Tables given by Chambers, (*Math. Tables*, by Bell) we get a refraction at the visible horizon of about 32′·0″., by using Ivory's Tables we get about 31′·30″. Bessel's form is not available as he introduces tan. zen. dist. which of course at the horizon is infinite. Allowing 9″ for horizontal parallax, and knowing my local time exactly, or within a very few seconds, the first limb agreed with theory to 9″, the second was 2′·43″ too early. By Ivory the first limb was 21″ too late, the second 2′·13″ too early. By too early, I mean that the Sun's limb had traversed that amount of arc more than was to be expected; and that it was in advance of its theoretical place. I should mention that the four occasions referred to were the only ones on which I was able to observe an exact Sunset in the course of a voyage of perhaps 12,000 miles.

(6) *On Critical or "Apparently Neutral" Equilibrium.* By J. Larmor, M.A.

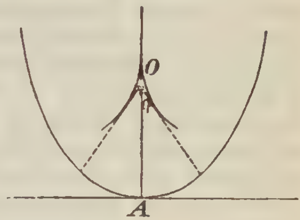
1. When a solid body is resting on a fixed surface, its equilibrium is stable when its centre of gravity is vertically below the centre of curvature at the point by which it rests, and unstable when vertically above it: when the two points coincide the equilibrium is often said to be apparently neutral, and its real character is discriminated by an analysis of the differentials of higher orders. It may be worth while to trace the origin of this peculiarity, and its practical effect on the nature of the equilibrium in cases which approximate to this critical condition.

2. Let us take the case of a heavy body symmetrical about two principal planes through its axis AB, (one of them the plane of the figure), and resting on a horizontal plane at A. The evolute of the section has a cusp at O, the centre of curvature corresponding to A. Let us suppose it to point downwards, so that the radius of curvature is a minimum at A, and let us suppose the centre of gravity G to be a very short distance above O. The position of the body is unstable, but a stable position exists in immediate proximity on each side, in which the tangents from G to the evolute are vertical. We see therefore, that when left free the body will oscillate at first round its upright position, and will finally settle down in one of these two slightly inclined positions. When G moves down to O, these two flanking stable positions come nearer to the upright position, and finally come up to it, so that the equilibrium is



really stable. But there is this peculiarity, that its oscillations round the vertical are no longer approximately Simple Harmonic, but follow another law which we can easily investigate, and that they are executed with extreme slowness: and we can trace the change to this new law from the rocking motion which is compounded of oscillations round the two flanking stable positions alternately.

3. If the cusp pointed upwards, and G were a very short distance below O, we would have a vertical stable position flanked by two very near positions of instability: and so, when G moves up to O, the vertical position becomes unstable. It is important, then, to bear in mind, that cases which satisfy the condition of stability, but are near to the critical case, are practically unstable for oscillations of any considerable amount, when the radius of curvature is a maximum at A.



4. These considerations clearly apply to all cases of critical or "apparently neutral" equilibrium, so that the determination of its real character carries with it the determination of the *practical* character of all other cases which approximate to that condition.

5. In the case of a floating body this discrimination is easy. If we consider, as usual, oscillations in which the displacement is constant, the centre of gravity of the displacement traces out a surface called the Surface of Buoyancy, and we know that the tangent plane to this surface corresponding to any position of the oscillating body is always horizontal, and that therefore the resultant fluid pressure acts along the normal at its point of contact. The circumstances of the oscillation are therefore the same as those of a solid body with the same centre of gravity, but bounded by the surface of buoyancy, and rolling on a frictionless horizontal plane. The evolute of the section of the surface of buoyancy is the locus of the metacentre, and has been called the Curve of Stability: and a case of equilibrium which approximates to the critical condition will come under § 2 or § 3 according as this curve has its cusp pointing downwards or upwards. Now the radius of curvature of the surface of buoyancy is known by the ordinary theory to be equal to the moment of inertia of the corresponding plane of floatation about an axis through its centre of gravity perpendicular to the plane of displacement, divided by the volume of the displacement: and therefore the case comes under § 2 or § 3 according as this moment of

inertia increases or diminishes as the degree of heeling increases, a criterion usually easy of application.

6. In a very numerous class of cases we can completely determine by geometry the curves mentioned above. Since any surface of the second degree may be derived by successive orthogonal projections, real or imaginary, from a sphere, it follows that the locus of the centre of gravity of a constant volume cut off from it by a plane is a similar and concentric surface. Hence for all this class of surfaces, which includes quadrics, cones, cylinders, pairs of planes, the surface of buoyancy is similar to the bounding surface of the floating body. Now the radius of curvature of a parabola or hyperbola is least at the vertex, and that of an ellipse is least at the ends of the major axis, and greatest at the ends of the minor axis: hence for paraboloidal and hyperboloidal surfaces, and cones, and wedges, the critical position is really stable, and for surfaces of which the section in the plane of displacement is elliptic it is stable or unstable according as the major or minor axis is vertical.

Further, if the shape near the water line come under any of these heads, the above conclusions clearly all apply, irrespective of the shapes of the other parts of the body.

7. We shall now investigate, as a typical case, the nature of the oscillations of a body whose section at the vertex approximates most closely to that of the parabola $y^2 = ax$, which rolls on a rough plane, and in which G is a very short distance c above O.

The equation of the evolute near the cusp is of the form

$$y^2 = \frac{16}{27a} x^3,$$

and when it is displaced through a small angle θ, the distance of the vertical tangent to the evolute from G is easily seen to be equal to

$$\frac{a}{4}\theta^3 - c\sin\theta,$$

to the third order in θ.

Thus, if κ represent the radius of gyration of the solid round an axis through its vertex, the equation of motion is

$$\frac{\kappa^2}{g}\frac{d^2\theta}{dt^2} = c\left(\theta - \frac{\theta^3}{6}\right) - \frac{a}{4}\theta^3,$$

to the third order in θ.

And the flanking positions of stable equilibrium are given by

$$\theta = \sqrt{12c/(3a+2c)}.$$

This equation is of the form

$$\frac{\kappa^2}{g}\ \frac{d^2\theta}{dt^2} = c\theta - m\theta^3,$$

where c is very small compared with m, which may be put equal to $\frac{1}{4}a$; and in fact we are investigating the character of oscillations under a restoring force proportional to the cube of the distance disturbed by a small force proportional to the distance.

8. We have

$$\frac{\kappa^2}{g}\left(\frac{d\theta}{dt}\right)^2 = \tfrac{1}{2}m\beta^4 - c\beta^2 + c\theta^2 - \tfrac{1}{2}m\theta^4,$$

where β is the amplitude of the swing; and therefore

$$t = \int \frac{\kappa d\theta}{g^{\frac{1}{2}}\sqrt{A + c\theta^2 - \frac{1}{2}m\theta^4}}$$

$$= \frac{\kappa}{\sqrt{Ag}} \int \frac{d\theta}{\sqrt{(1-p\theta^2)(1+q\theta^2)}}$$

$$= \frac{\kappa}{\sqrt{Apg}}\sqrt{1-k^2}\int \frac{-d\phi}{\sqrt{1-k^2\sin^2\phi}},$$

where $\quad \cos\phi = \sqrt{p}\,.\,\theta,\quad k^2 = \dfrac{q}{p+q},$

$$A = \tfrac{1}{2}m\beta^4 - c\beta^2,$$

$$q - p = \frac{c}{A}, \text{ which is small,}$$

$$qp = \frac{m}{2A};$$

so that $\quad q + p = \sqrt{\dfrac{2m}{A}}\left(1 + \dfrac{c^2}{4mA}\right)$, approximately.

Also $\dfrac{c}{m\beta^2}$ is a small quantity which we shall call e.

Hence $\quad t = \dfrac{\kappa}{g^{\frac{1}{2}}\sqrt{A(p+q)}}\displaystyle\int \frac{-d\phi}{\sqrt{1-k^2\sin^2\phi}}$

$$= \kappa\,(mg\beta^2)^{-\frac{1}{2}}\left(1 + \frac{c}{2m\beta^2}\right)\int \frac{-d\phi}{\sqrt{1-k^2\sin^2\phi}}$$

$$= \kappa\,.\,(mg\beta^2)^{-\frac{1}{2}}(1+\tfrac{1}{2}e)\left\{F\left(\frac{\pi}{2}, k\right) - F(\phi, k)\right\};$$

where $\quad \cos\phi = \sqrt{p}\,.\,\theta = \dfrac{\theta}{\beta}(1 - \tfrac{1}{2}e),\quad k^2 = \tfrac{1}{2}(1+e).$

Now write $\cos\psi = \frac{\theta}{\beta}$, and use the results for differentiating F that are given in Cayley's *Elliptic Functions*, § 73. We find, on putting $\phi = 0$, that a quarter period of the oscillation is given by

$$T = \kappa (mg\beta^2)^{-\frac{1}{2}} (1+\tfrac{1}{2}e) \left[F\left(\frac{\pi}{2}, \frac{1}{\sqrt{2}}\right) + e\left\{E\left(\frac{\pi}{2}, \frac{1}{\sqrt{2}}\right) - \tfrac{1}{2}F\left(\frac{\pi}{2}, \frac{1}{\sqrt{2}}\right)\right\}\right]$$

$$= \frac{\kappa}{g^{\frac{1}{2}} m^{\frac{1}{2}} \beta} [F_1 + eE_1],$$

in which $F_1 E_1$ stand for the complete elliptic integrals of the first and second orders to modulus $\sin 45^\circ$, and $e = c/m\beta^2$.

Also

$$t = T - \frac{\kappa}{g^{\frac{1}{2}} m^{\frac{1}{2}} \beta} (1 + \tfrac{1}{2}e) \left[F + e(E - \tfrac{1}{2}F) - \tfrac{1}{2}e \sin\psi\cos\psi + \frac{e\cos\psi}{2\sin\psi\sqrt{1-\frac{1}{2}\sin^2\psi}} \right]$$

$$= T - \frac{\kappa}{g^{\frac{1}{2}} m^{\frac{1}{2}} \beta} \left[F + eE - \frac{e}{2\beta^2}\theta\sqrt{\beta^2-\theta^2} + \frac{e\beta\theta}{\sqrt{2(\beta^4-\theta^4)}} \right],$$

in which F, E are the functions of $\operatorname{arc}\cos\frac{\theta}{\beta}$ to modulus $\sin 45^\circ$, whose values can be taken at once from Legendre's tables.

9. The disturbance produced by the small term proportional to the distance is represented by the terms multiplied by e. And, in particular, if $e = 0$, so that the equilibrium is critical, we have

$$T = \frac{\kappa . F\left(\frac{\pi}{2}, \sin 45^\circ\right)}{g^{\frac{1}{2}} m^{\frac{1}{2}} \beta},$$

$$t = \frac{\kappa}{g^{\frac{1}{2}} m^{\frac{1}{2}} \beta} \left\{ F\left(\frac{\pi}{2}, \sin 45^\circ\right) - F\left(\operatorname{arc}\cos\frac{\theta}{\beta}, \sin 45^\circ\right)\right\},$$

where $m = a/4$, β = amplitude of excursion.

In this case, therefore, the period of an oscillation varies inversely as its amplitude, and is equal to

$$\frac{2\kappa}{g^{\frac{1}{2}} a^{\frac{1}{2}} \beta} \times 1{\cdot}85407.$$

If the plane on which the motion takes place be frictionless, κ will be the radius of gyration about an axis through the centre of gravity: and the same will be the case in the problem of hydrostatic oscillations.

10. In the case of one sphere resting on another it is very easy to form the equation of energy, and thence determine the value of m. In the case of one symmetrical body resting on another, whether on the summit or not, we have, if θ be the angle by which G overhangs the vertical through the point of support,

$$\frac{\kappa^2}{g}\frac{d^2\phi}{dt^2}=\left(\frac{d^3\theta}{ds^3}\right)_0\frac{s^3}{6}$$

in the critical case; therefore

$$\frac{d^2s}{dt^2}=\frac{g}{6\kappa^2}\left(\frac{1}{\rho}+\frac{1}{\rho'}\right)^{-1}\left(\frac{d^3\theta}{ds^3}\right)_0 s^3,$$

in which the value of $\left(\frac{d^3\theta}{ds^3}\right)_0$ is given by Mr Routh in the *Quarterly Journal of Mathematics*, XI. p. 106; and thus the value of m is determined. The nature of the equilibrium in the critical case appears to have been first discussed by Dr Curtis, *Q. J.* IX. p. 42.

11. The exception to this critical case again, is that in which five positions of equilibrium come together. Then the equation of motion is of the form

$$\frac{d^2\theta}{dt^2}=-\mu\theta^5.$$

Hence
$$t=\sqrt{\frac{3}{\mu}}\cdot\int\frac{d\theta}{\sqrt{\beta^6-\theta^6}}$$
$$=\sqrt{\frac{3}{\mu}}\cdot\frac{1}{\beta^2}\int\frac{d\phi}{\sqrt{1-\phi^6}}.$$

And on writing
$$\phi^2=\frac{1}{1+y^2},$$
$$y=3^{\frac{1}{4}}\tan\frac{\psi}{2},$$

this reduces to (*vide* Bertand's *Integral Calculus*)

$$t=\sqrt{\frac{3}{\mu}}\cdot\frac{1}{\beta^2}\cdot\frac{1}{3^{\frac{1}{4}}}\int\frac{d\phi}{\sqrt{1-\sin^2 15^\circ\sin^2\psi}},$$

and a quarter period is given by

$$T=2\cdot\frac{3^{\frac{1}{4}}}{\mu^{\frac{1}{2}}\beta^2}\times 1{\cdot}59814,$$

which varies inversely as the square of the amplitude.

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